

Upheaval Ahead for the European Community

THE enlargement of the European Community in January next year is likely to be as traumatic for the nations which already belong as for the three which will then become members. So much at least should be evident from even the balanced arguments which appear to have decided the Norwegians to withdraw their application for membership. The referendum this week in Denmark, and the talk which has dominated the Labour Party conference at Blackpool, are further proof that it will be hard for the organization in Brussels to pursue the policies with which it has all too quickly become familiar. In Norway, the short-term economic disadvantages of membership, especially the requirement there would have been that other nations' fishermen should have access to Norwegian fishing grounds, appear to have been more influential than calculations of long-term economic gain, while a narrow majority of the voters there seem also to have been affronted at the prospect that membership would entail unwelcome changes of traditional ways and a further exacerbation of the present sense of isolation that the communities in the north appear to suffer—it is about as far from Trondheim to Oslo as from Brussels to Rome, so that the Norwegian decision to pull out is entirely understandable. The reservations of the Blackpool conference of the Labour Party, less rational though they may be, are also, however, a shadow over Brussels. The problem is whether the applicants for membership should stomach the present defects of the European Community for the sake of the benefits of a customs union. There is no harm in anticipating some of the things the new members will have to say.

Only last week, Mr Patrick Jenkin, representing the British Treasury in Brussels, showed which way the wind will blow. Among other things, the Treasury appears to object to the requirement that it should pay a contribution to the cost of the Euratom organization which has somehow managed to survive like a coelacanth since the days when the European Communities believed that European nuclear energy could be managed centrally. If Euratom had been better managed at the beginning, the European Commission's hopes might have been realized, and it is not even too late to think that something useful might be done to coordinate European research and development in nuclear energy; but except where nuclear safeguards are concerned, Euratom is a dead duck and should be recognized as such. From next January onwards, there are also likely to be vigorous complaints at the present running of the common agricultural policy—the principle that there should be a European agricultural industry with uniform market prices may be acceptable—if only just—as a short-term measure, but the present arrangements are so sanctified by the bureaucracy in Brussels that they are not flexible enough to meet changing circumstances nor dynamic enough to encourage the most economical distribution of agriculture within Europe fifty years from now. The failure of Euratom

is an example of how farsighted institution building can be mishandled. The agricultural policy shows how the six founder members of the European Community have sought misguidedly to achieve integration without change. The task for the constructive critics of the European Community is to thread a course between these quite different kinds of dangers.

The first thing to acknowledge is that the European Community has no permanent future as a customs union pure and simple. Indeed, the Treaty of Rome lays down that there should be free movement of labour and capital within the European Community, which implies that in due course both people and money will congregate where it is economically most advantageous for them to do so. What this implies is that such short-run economic advantages as there may be in membership will ultimately be outweighed by the benefits of social integration. If this is the goal, however, it is a matter only of timing to know when political integration must follow, at least in some important areas of public policy. Why, for example, should the members of the community allow the European Commission to run its policy of help for depressed regions in Europe according to some bureaucratic rule of thumb when it is plain that a decision to provide help for the Mezzogiorno or the Scottish Highlands requires not merely a political judgment by the rest of Europe of the urgency with which help is needed but some vision of what it hopes eventually to accomplish by such a policy. (In the long run, for example, it might be considered preferable that these and other regions should remain rural as they are at present rather than become industrialized, which implies that regional help should be a temporary expedient.) How will this kind of argument determine European policy in science and technology?

The problem is to devise ways of insulating member countries from the frequently painful consequences of immediate change while planning for a future, three or four decades ahead, in which integration is so complete that the benefits of scale can be enjoyed by everybody. The universities are a good place to start, if only because change is likely to be slow. And given the principle, sometimes an illusion, that universities are self-governing communities of scholars, is there not a case for asking that all countries within the European Community should agree that each other's nationals can make careers at any European university? Since many European university systems, the French for example, require that university teachers should be civil servants, this would require a considerable loosening of present practice on the European mainland, but the benefits even in the immediate future would be considerable. If the eventual consequence were the disestablishment of European universities, no tears would be shed.

The European Commission's own proposal for a European Science Foundation would work in the same direc-



tion. The idea, as outlined in the commission's working paper at the end of May (see *Nature*, **237**, 242; 1972), is that there should be a modest organization charged with the encouragement of scientific exchanges on a European basis and able to advise on the development of other European institutions such as CERN or ESRO. It remains an open question whether the new foundation should cut across the bilateral schemes which have grown up in the past few years for the transfer of people from one country to another, but there is without question useful work to be done. And in the long run, if the foundation can avoid the errors of over-ambition and bureaucracy, it could become a channel by means of which all European governments would finance at least a part of basic research in Europe.

Industrial research is a bigger headache. The failure of Euratom was largely a failure to persuade European governments and public utilities that Euratom could satisfy a common interest in the development of nuclear energy. A part of the trouble, in the late 1950s, was the French decision to manufacture nuclear weapons, but Euratom also failed to persuade its potential beneficiaries that they could abandon their own national programmes once the common research policy had been agreed. Both in the development of fast reactors and of devices for thermonuclear fusion, however, there remains an urgent and important need for common research and development. The difficulty is that the commercial exploitation of fast reactors is now too close for a centrally funded research organization to be feasible—but such a device, in any case, would probably fall into the errors which Euratom made. The best hope is that the atomic energy authorities and the electricity utilities of Europe, the only customers for fast reactors now in sight, should organize a less formal programme of collaboration. Is it too much to ask that the United Kingdom Atomic Energy Authority should take the initiative? There is everything to be gained and nothing to be lost, for the experience of the past few decades has shown clearly enough that reactor sales are closely linked with national considerations. But in any case, the hesitations in the European Commission's own pronouncement on research and development show quite clearly that it will be best if European technology can be coordinated not from Brussels but by the evolution of institutions in which existing research organizations, the manufacturers with whom they work and the potential customers are intimately involved. **If one advantage of such an arrangement is that the bureaucracy of Brussels is not directly concerned, so much the better.**

The same pattern aimed at the involvement of interested parties could with advantage be applied in other fields, telecommunications for example. As things are, each of the members of the European Community has its own publicly owned telecommunications authority, some of which have been able to lift their eyes above the immediate goal of meeting the public demand for connexion to the telephone system (although it seems still to take the best part of a year for a would-be telephone subscriber in Paris to be connected to the system). Here again the problem is how best to chart a course between the immediate future, when the several authorities will be anxious to maximize their national interests, and the distant future when there will no doubt be a single European telecommunications authority responsible for planning

the development of the system as a whole, both technically and in terms of capital investment. The first need is that the long-term integration of a European telecommunications network should not be hampered by the installation in the next few years of incompatible systems, which argues for an urgent agreement on the technical objectives now implicit in programmes of research and development such as that of the British Post Office, which has set its heart on a microwave trunk network for the 1980s using circular wave guides as well as atmospheric links. Will it not be ironical if it turns out, some years from now, that by neglect of the opportunities which exist for coordination, the European telecommunications organizations have embarked on quite different schemes? But as things are, the genteel consultation between public authorities is no substitute for a common technical policy and much the same is true of policy—or the lack of policy—in the air transport industry and in electricity generation. This is why it must be for European governments urgently to ensure that their publicly owned utilities are forced into arrangements for common planning. The best way of doing this would be to have them all accept that purchasing policies should quickly be freed from the chauvinistic habits of the past. The British Post Office, for example, should be required to throw its business open to all potential suppliers in the European Community, Siemens included.

Government by Halves

IF Lord Rothschild's recipe for the conduct of research and development in Britain is to function properly, the government departments that will in future act as the customers for applied research must be effective customers. So much has been apparent from the outset. It was, for example, one of Lord Rothschild's own reservations. Since last October, government spokesmen have repeatedly endorsed this view. But there are unhappy signs that the government departments are hard at work watering down the doctrine that a good customer must first of all know his own mind clearly. In this sense, the Institution of Professional Civil Servants has a just complaint at the new arrangements being set up within the Ministry of Agriculture, Fisheries and Food, which happens to be the largest beneficiary under the government's proposals for transferring funds from the research councils to government departments. The plan is that there should be a Chief Scientist, but that he should become a kind of adviser to the Research and Development Division which the ministry set up in 1971, and which is at present largely occupied with the administration of the ministry's own laboratories. Executive responsibility for placing contracts for applied research to begin with at the research councils but possibly later on, with other organizations, will rest with the peripatetic civil servants now in charge.

What the ministry proposes is therefore a peculiarly British device for getting the worst of all worlds. It is a device for separating responsibility for making technical judgment from responsibility for backing technical advice with money. Especially at a time when the government is supposed to be trying to incorporate the principles of modern management into the organization of the civil service, it is mystifying that the Ministry of Agriculture

Fisheries and Food should have seriously advocated such an awkward structure. Who, under the new arrangements, will be to blame if an unproductive research programme is blessed with ministry money? Who, if anybody, will be responsible if imaginative proposals are turned down for administrative or budgetary reasons? Under the new arrangements, the chances are that the Chief Scientist and his administrative superior will be best of all equipped to shift the blame from one to another. The result will be a research programme which is less sharp than it might have been and which is, for that reason, an assurance that Britain will benefit less from the merits of the Rothschild recipe than it might have done. Another unpalatable feature of the arrangement is that the ministry appears to be bent on perpetuating the old tradition that scientists should be kept out of the line of managerial responsibility.

Unhappily, the government's response to its own white paper, published nearly three months ago, is equally laggard in other fields. In the Department of Trade and Industry, for example, where the customers are to be represented by a series of what are called Requirements Boards, progress seems to be painfully slow. So far, the department has not been able to announce the formation of a single board. The more charitable of its critics may suppose that there are second thoughts about the wisdom of this strange device for creating within the same department of government organizations which represent customers (the requirements boards) and organizations which represent contractors (most of whom will be the department's own laboratories). But it is also possible to regard the quite inexcusable delay since the requirements boards were first mentioned in the early summer as a measure of the department's unwillingness urgently to reconstruct its own machinery for making decisions on research and development.

Broadening the PhD

THE Science Research Council and the Social Science Research Council have made a useful if so far modest approach as to how best to devise ways of broadening postgraduate courses. Four years ago, the two research councils set up a joint committee to give advice on the development of courses and the making of awards for the students concerned, with Professor H. Kay of the University of Sheffield as chairman. The committee has now produced an informal report on the subject (*Broader Education for Graduates*, free of charge from the SRC) which starts from the premise that the traditional PhD course is well designed to equip graduates for research work but that it is too narrow for those who follow careers in development, technical planning and production. So the committee has set out to identify opportunities for developing more broadly based PhD courses which would be "intellectually as demanding as the traditional PhD . . . but more directly oriented towards industrial careers". The committee says that the most compelling need for a more broadly based PhD course is the need "to use the enormous resources derived from the discoveries of science and the developments of technology in ways that give the maximum benefit and the minimum harm to the community". Its successes so far include the sponsorship of what is called the interdisciplinary higher degree at

Aston University, an industrial PhD at Loughborough University, an integration of science and economics at the PhD level at the University of Stirling, and schemes for linking postgraduate study with, for example, studies in science policy at a handful of other British universities.

Although the numbers of studentships awarded under this scheme amounted to only 38 in 1971, this is not a field in which anybody would look for rapid progress. The joint committee itself acknowledges some of the more obvious doubts about the broadening of PhD instruction. In particular, it raises the question whether its objectives might not be more easily attained and more permanently secured by a broadening of undergraduate education in British universities, still well on the way to being the most specialized in the world, at least where the brighter students are concerned. The committee is probably right in thinking a more broadly based postgraduate course would help enormously to influence university departments and their teachers in beneficial ways, but unhappily it takes the view that for the "foreseeable future" the tendency to specialize at the undergraduate level will be predominant. Need this be so? Are university courses in Britain so immune to reform? Is there not in any case a likelihood that the brighter undergraduates, those most likely to finish up in PhD courses, will seize such options as are available for broader undergraduate courses? These issues need more careful attention.

At the postgraduate level, there remains a need to know more about the quality of such integrated PhD courses as exist. The danger, of course, is that many attempts at broadening will consist of a traditional PhD course garnished with superficial studies in other disciplines. On the principle that a little learning can never be dangerous, developments like these would no doubt be advantageous to the people concerned. Whether they would be similarly beneficial to employers and to the community is a more open question. It is always possible that the snobbish influence of what might be called PhD-ism might create the illusion that some intellectual magic has been performed. Evaluation should be the next step.

100 Years Ago



THE Commissioners of Her Majesty's Works and Public Buildings intend to distribute this autumn, among the working classes and the poor inhabitants of London, the surplus bedding-out plants in Battersea, Hyde, the Regent's, and Victoria Parks, and in the Royal Gardens, Kew. If the clergy, school committees, and others interested, will make application to the Superintendents of the Parks nearest to their respective parishes, or to the Director of the Royal Gardens Kew, in the cases of persons residing in that neighbourhood, they will receive early intimation of the number of plants that can be allotted to each applicant, and of the time and manner of their distribution.

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OLD WORLD

IPCS Attacks White Paper

THE silence which has enveloped the British scientific community since the publication of the government white paper on research and development last July was broken yesterday by the publication of a scathing attack on the government's proposals by the Institution of Professional Civil Servants.

The white paper, says the IPCS, is an "inadequate and disappointing response to the wide-ranging public debate about [Lord Rothschild's] formulations". In particular, the institution complains that no reference is made to strategic research by the government when it recommends that £20 million should be transferred from the research councils to government departments over the next three years. This "deplorable" omission, says the IPCS, implies that the government has failed to "grasp one of the essential functions of the research councils".

The institution is also unhappy about the statement in the white paper that "no conditions are to be placed on the use of the money transferred to the customer departments but the expectation is that it will be spent to commission applied research work from the research councils". Such an approach will lead to uncertainty within the research councils, says the IPCS, and to prevent this the institution strongly urges the government, as the Select Committee on Science and Technology has done, to plan future departmental spending on research and development on a five year basis.

The IPCS is also unhappy at what it considers a toning down of a promise made by Lord Jellicoe in the House of Lords in February this year during a debate on the green paper on research and development. Lord Jellicoe said that the government had "a closed mind on the principle that no transfer should take place unless and until the government departments concerned are properly equipped and capable of managing the funds entrusted to them". In the white paper the government merely states that no transfers of money will be made "until the customer departments have established their central scientific staffs".

The Civil Service Department, however, denied this week that the white paper was inconsistent with what Lord Jellicoe had said in February and that it was still the intention of the government to ensure the efficiency of each department before any money was transferred.

The institution also regrets the division of the Nature Conservancy, but says that the new arrangement will function if those responsible for the organization of the conservation work in the new Nature Conservancy Council and for the research work in the Natural Environment Research Council "get on well together and are willing and anxious to assist the work of the other section".

On the positive side, the institution considers that the white paper's recommendation that £20 million should be transferred to the research councils over three years and not immediately is a merit. It also likes the government's reaffirmation of its commitment that scientists should play a larger role in working out departmental needs and in formulating policy. The institution also welcomes the setting up of a task force to advise the government on how to encourage greater mobility within the civil service but it regrets that it was not represented on this task force.

The IPCS welcomes the government's assurance that after the £20 million has been transferred to the departments in 1975-76 there will be a "period of stability before any further review is carried out". The institution has been

concerned about the effects that frequent reviews have on the morale of the staff in government laboratories.

In spite of the detailed criticisms, the institution pledges its support for the new system, which it says will "ensure an effective link between science and the community's needs". The IPCS also welcomes the government's unequivocal commitment "to the maximum cooperation between all the parties involved" and on this basis alone, says the institution, "the government's new organization could turn out to be a turning point".

ATOMIC ENERGY

International Fusion

IN spite of speculation last year that the United Kingdom Atomic Energy Authority was in danger of being disbanded, the annual report for the year ending in March 1972, published this week, shows no outward sign of this (*United Kingdom Atomic Energy Authority*, 18th Annual Report and Accounts, 1971-72).

The authority's expenditure during 1971-72 was £96.59 million, compared with £95.3 million in 1970-71. The authority recouped £51.2 million of this

RESEARCH AND DEVELOPMENT

Leaden Shoes

GOVERNMENT departments are making slow progress in bringing their research and development organizations into line with last July's white paper. Although the transfers of funds from research councils to ministries outlined in the white paper do not become effective until March 1973, the Chief Scientists' organizations, which Lord Jellicoe has repeatedly said will have to be firmly established before funds are transferred, are still in a very early stage.

The Department of Health and Social Security seems to have progressed furthest, having appointed Dr R. H. L. Cohen as chief scientist until April 1973 and Professor Douglas Black of the University of Manchester as his successor. The "small team of scientists"—thought to be about a dozen—that will complete the organization has not been appointed, however, and formal talks with the Medical Research Council on which projects the DHSS will take over from the MRC have not yet begun.

The situation at the Department of Trade and Industry seems equally un-

decided. The requirements boards which were paraded before the Select Committee on Science and Technology even before the white paper's publication have still not been set up.

Things seem little better at the Ministry of Agriculture, Fisheries and Foods where a Chief Scientific Adviser (Agriculture) has been appointed in the person of Mr W. F. Raymond, formerly of the Grasslands Research Institute, and where Mr G. Elton, already a member of the department has acceded to the title Chief Scientific Adviser (Food). A similar small team of scientists—thought to number about six at Senior Scientific Officer level or higher—has yet to be appointed. The Chief Scientist has not yet been named, although the name of Dr H. C. Pereira of the East Malling Research Institute has been mentioned in the past few weeks.

Only the Department of the Environment seems so far equipped to handle its Rothschild money, £2.25 million in the year beginning next March. An organization consisting of a Director General (Research) with a supporting staff was set up in 1970 when the department was first created.

in receipts and £45.3 million was supplied by parliamentary grant. During the current year, however, net receipts are predicted to be lower at £37.9 million, the drop resulting chiefly from the fact that the final Defence Settlement payment of £19.4 million was made last year. The predicted total expenditure is also less this year and the shortfall to be made good by parliament will amount to only £4.3 million more than was paid last year.

The authority spent £46.6 million on reactor research and development during 1971-72, £4.7 million more than in the previous year. The increase came chiefly in expenditure on the fast reactor system which rose from £26.3 million to £30.2 million. The cost of other research carried out by the authority amounted to £61.6 million, £4.1 million more than in 1970-71.

In 1970 it was planned that the first fast reactor commercial station would be operational in 1974, but in the report the authority says that a "somewhat longer timetable" is now likely. This delay stems from a decision taken after the initial plan was published to develop the first station to "a stage at which it will be suitable for replication". The actual date on which the first fast reactor will become operational will, according to the report, be dictated by the performance of the prototype fast reactor during its first year of operation. But the authority reiterates its faith in the fast reactor, and during the past year it has carried out detailed estimates on the cost and performance of thermal and fast reactors which justifies continued development.

Nuclear fusion and plasma research last year was financed to the tune of £4.4 million, £0.3 million more than in 1970-71. But the authority's programme at the Culham Laboratory continues to diminish as it has done since 1967. But in spite of the decreased financial support in real terms, the authority is confident "that the conditions needed for controlled fusion reactions can be attained".

The authority sees the future of fusion research on an international rather than on a national basis, and it points out that the arrangement between Euratom and Culham, set up in June 1971, has been successful. Under this arrangement Euratom contributed \$46.5 million towards a \$180 million fusion programme which incorporates the work of all the fusion laboratories within the European Economic Community. With Culham already partly involved in international fusion work, the authority feels that some kind of contract of association is needed between the authority and Euratom. Preliminary discussions about this have already taken place. The authority boldly states that such a contract would

provide the framework in which to establish a European project leading to fusion power for western Europe in the 21st century.

WATER RESOURCES

Fly in the Ointment

BRITAIN'S rivers are getting cleaner. The latest version of the River Pollution Survey which is to be published shortly will reveal that one mile in ten of the rivers that were classified as heavily polluted two years ago are now cleaner than they were, and that 77.5 per cent of non-tidal rivers are now free from pollution, compared with 72 per cent in the late 1950s. These figures were disclosed by Mr Eldon Griffiths, Under-Secretary of State at the Department of the Environment, when he opened a conference on the management of national and regional water resources in London this week. The figures, Mr Griffiths said, represent the overall picture, and some rivers have deteriorated; but for every mile of river that has become more polluted, six miles are now cleaner than they used to be.

Mr Griffiths went on to emphasize that the details of the government's plans to reorganize Britain's water industry have not been settled although the government is adamant that "henceforth the whole water cycle must be, and will be, managed as a whole". The full discussion that has followed the circulation of consultation documents on the proposed changes has led to changes in the government's attitude to some of its proposals, Mr Griffiths said, largely in the relationship between the ten new regional water authorities (RWAs) and local government.

But others at the conference clearly felt that, while the proposal to replace the existing 29 river authorities and myriad smaller bodies concerned with drainage, sewerage and recreation with ten large RWAs is a sound one, the absence of a strong central body could prove a serious weakness in the new system. Under the new proposals, the Water Resources Board will cease to exist, and the central body will be known as the National Water Council but will have no executive powers. Planning will be run by a Central Water Planning Unit within the Department of the Environment, and research by a new water industrial research centre based on the Water Research Association, with a Water Space Amenity Commission advising on recreational use of water.

This structure has been proposed because the government feels that with only ten large regional water authorities, the need for central day to day control will be reduced and that any powerful body placed between the Department of

the Environment and the Regional Water Authorities would only hinder decisions on large-scale water schemes.

Dr H. H. Cann of the Dee and Clwyd River Authority disagreed with this view. The planning unit should be a technical arm of the National Water Council, he said, and not an integral part of the Department of the Environment. It should be seen to be independent of government so that its assessments of the technical feasibility are independent of politics.

Mr B. Rydz, the Water Resources Board's assistant director, also put in a plea for a central body with some real authority, pointing out that all reforms of Britain's water industry so far have been directed towards such a body. Further, both the Central Advisory Water Committee, which spent eighteen months studying the problems of Britain's water industry, and the Proudman Committee (a subcommittee of CAWC) were adamant that a strong central body must be created.

But the chances of the National Water Council actually acquiring a bite to go with its advisory bark seem remote. Mr Jack Beddoe, Under Secretary at the Department of the Environment in charge of the reorganization, held out little hope of a change of heart.

Reform Unchanged

THE Reform Club in London celebrated this week the placing of Mr Phineas Fogg's fictional wager to travel around the world in eighty days, said by Jules Verne to have taken place at 7.45 p.m. on October 2, 1872, at the Reform Club, with a banquet at Guildhall. Professor Jean Chesneaux from the University of Paris marvelled at Jules Verne's early faith in the wonders of technology as well as at the way he combined liberality with xenophobia, but described his later disillusion with scientific things as a justifiable model for the disenchantment of the modern world. Sir Denis Brogan, an historian, was equally impressed with the limitations of technology. Sir Bernard Lovell, however, had the happy thought that relativistic time dilation might enable some future Phineas Fogg to circumnavigate the galaxy in eighty days. On balance, the Reform Club, dedicated to opposition to the reintroduction of the Corn Laws in the eighteenth century though it may be, succeeded in giving the impression that Mr Fogg would only with difficulty have been elected to the membership which so far includes no woman either.

NEW WORLD

Kennedy Bill meets Opposition and Runs Out of Time

by our Washington Correspondent

DR H. GUYFORD STEVER, Director of the National Science Foundation, went to Capitol Hill last week to complain about Senator Edward M. Kennedy's National Science Policy and Priorities Act (see *Nature*, 237, 306; 1972). Even though his remarks will have little effect on the immediate prospects for the bill—there is now almost no chance that it will reach the floor of the House of Representatives this year—they provided a clear indication of the Administration's thinking on its two key provisions: that the NSF should assume the leading role in trying to bring science and technology to bear on urban problems and that the Federal government should help to retrain unemployed scientists and engineers. Dr Stever reiterated the Administration's opposition to both proposals.

The forum for Dr Stever's complaints was two days of hearings held by a subcommittee of the House Committee on Science and Astronautics. The hearings also brought out a number of other interesting points. There was, for example, a marked difference between the testimony of Dr Roger Heyns, vice-chairman of the National Science Board (the policy-making body of the NSF), and that of Dr Stever. Dr Heyns and his colleagues on the board seem more amenable to the bill's proposals, which probably reflects the fact that Dr Stever's testimony was reviewed by the White House and therefore represents the election year thinking of the Administration. Another point that emerged at the hearings is that the bill has strong bipartisan support in the subcommittee on Science, Research and Development. This factor will be a key to the bill's fortunes if it is introduced next year, as now seems certain.

Dr Stever's chief complaint is that although the bill's objectives "are ones on which many reasonable men could agree", the bill itself is unnecessary. Senator Kennedy and his supporters are seeking to increase expenditures on civilian research and development directed towards improving services such as health care, urban transportation and pollution control, and they have proposed that the NSF should become the central point in the federal government for planning and executing such activities. But Dr Stever claimed that the Administration has already significantly increased expenditures on civilian science and that the projects encompassed by the bill are already under way, either

in NSF or in other mission-oriented agencies. "I believe that the Administration and the Congress are already taking constructive steps towards meeting the national needs perceived by (the bill's) sponsors," he told the committee.

Of particular concern to Dr Stever and the Administration is that the bill would involve the foundation in many projects that now fall under the responsibility of other agencies of the federal government. Policy planning, for example, is the responsibility of the Office of Science and Technology and the Office of Management and Budget, while "development, testing and evaluation of major civil science systems are, in most cases, more appropriately done in mission agencies responsible for implementing the systems".

But other witnesses did not agree. Dr Heyns, for example, approved the suggestion that the NSF should play a greater part in the development of national science policy, and agreed with some reservations that the foundation should be assigned the responsibility for developing, testing and evaluating civil science systems. He suggested that the foundation's relationship with the scientific community and its experience with the programme of Research Applied to National Needs (RANN) should stand it in good stead to carry out both tasks. The National Science Board's reserva-

tion, however, is that such new responsibilities should not be allowed to interfere with the foundation's chief role of supporting basic science.

As for unemployed scientists and engineers, Dr Stever repeated the Administration's policy that they should not be considered as special cases, but should wait for an upswing in the economy to lift them out of the dole queues. "Any solution to (unemployment) problems must include other groups of unemployed," he said, and in any case Dr Stever suggested that "the manpower problem is temporary". Support for this position has in fact already been provided by the Administration, because it has decided not to continue the Presidential Internship Programme which has provided temporary employment for some 560 scientists and engineers. White House spokesmen point out that the programme was only intended to last for one year, even though the job market has hardly improved. The bill, on the other hand, calls for a variety of programmes for retraining scientists and engineers, and for providing them with work on civilian projects.

The reason why the Administration has chosen to oppose the bill is relatively simple—the bill can be read as an indictment of the Nixon Administration's science policies. Dr Stever

INTERNATIONAL EXCHANGES

Visitors from China

by our Washington Correspondent

SINCE the Chinese table tennis team broke the ice between the People's Republic of China and the United States last year, the scientific community in the United States has been looking forward to exchanges between the two countries. But, although there has been a slow trickle of US scientists travelling East, so far no Chinese scientist has paid a return visit. Next week, however, as announced last week (*Nature*, 239, 245; 1972) a party of ten medical doctors from the People's Republic will arrive in the United States for a three week visit at the invitation of the Institute of Medicine, the American Medical Association and four American physicians who visited China last year.

The Chinese visit will include a look at hospitals, research institutions and other medical establishments in Washington, New York, Boston, Kansas City, San Francisco and perhaps Chicago. The Chinese visitors have

expressed a special interest in cardiovascular diseases and cancer. An invitation was sent to the China Medical Association in May, and details of the visit were negotiated by Dr E. Grey Dimond, of the University of Missouri, during a recent trip to Peking.

The Chinese party will consist of Wu Wei-jan, Vice-chairman of the Association of Surgery, All-China Medical Association, and Capital Hospital, Chinese Academy of Medical Sciences; Fu Yi-cheng, Deputy Secretary-General of the All-China Medical Association; Lin Chiao-chih, Peking Medical College; Wu Hsueh-yu, Hospital of the Wuhan Medical College, Hupeth Province; Li Ping, Jeh Tan Hospital, Chinese Academy of Medical Sciences; Liu Shih-lien, Chinese Academy of Medical Sciences; Han Jui, Chinese Academy of Medical Sciences; Chou Kuan-han, Peking Medical College; Chang Shu-shun, Peking Tuberculosis Research Institute; and Hsu Chia-yu, Tung Fang Hung Hospital, Shanghai. Some members of the delegation are believed skilled in traditional Chinese medicine.

suggests that the Administration is already taking steps to meet the objectives that Senator Kennedy seeks, but other witnesses believe that it is not moving fast enough, or effectively. In any case, the Administration probably will not hear much more about the bill this year, because the House Rules Committee has said that legislation that is not yet reported out of committee will not be considered by the House this session.

NAS

A Slight Tarnish

by our Washington Correspondent

ONE casualty of the recent hearings on food additives was the Food Protection Committee of the National Academy of Sciences, which was the subject of a stream of serious charges. The effect on the academy itself remains to be seen, but there can be little doubt that its image has been badly tarnished. The following are the more serious of the charges: Dr John Olney, a professor of psychiatry at Washington University School of Medicine, kicked off the assault on the Food Protection Committee by suggesting that a review of the possible health hazards associated with monosodium glutamate (MSG) which it conducted for the Food and Drug Administration "had too many characteristics of an industry-arranged whitewash affair". Specifically, he charged that the members of the subcommittee which reviewed the MSG data had "a high degree of industry bias and poor qualifications to judge the issue under consideration"—only one member of the committee was primarily a neuropathologist. Moreover, he charged that the chairman of the subcommittee, Dr Lloyd J. Filer, jun, was conducting research on MSG under contract from Gerber, the baby foods company, while the committee was considering the issue.

"It would appear that these industries, by providing (Filer's) research group with money while he chaired the subcommittee, may have influenced his ability to make objective decisions," Olney suggested. He also charged that other members of the committee had strong attachments to the food industry, in particular Dr Lloyd W. Hazelton, who was a member of the subcommittee, but who was the president of an industrial research laboratory which does a substantial amount of its work for the food industry.

The second major attack on the Food Protection Committee came from Dr Samuel Epstein, of Case Western Reserve. Epstein took issue in particular with a report produced by the committee in 1969 which was strongly critical of the Delaney amendment. He

charged that of the nine member task force which produced the report, "five were industrial employees, one a commercial testing house director and the other three academic scientists with long and intimate associations with industrial interests . . . none is recognized as knowledgeable, let alone as an authority, in the field of carcinogenesis". He suggested that the close association of the committee with industrial interests "makes it singularly inappropriate as a major source of 'independent' advice on consumer safety to the FDA, a role which it has exercised so forcefully over the last decade".

Ms Anita Johnson, an attorney with the Health Research Corporation, also took issue with the Food Protection Committee for a report it published recently on a colouring agent called Red No. 2. She said, "The committee has no NAS members on it. It is well known for its bias for industry affairs at the expense of consumer protection . . . and has consistently received financial support from the food, packaging, and chemical industries."

Perhaps the most damaging indictment at the hearings came from Dr Charles C. Edwards, commissioner of the Food and Drug Administration. Senator Charles C. Percy asked Edwards whether the FDA chose the Federation of American Societies for Experimental Biology to research the list of food additives that are generally regarded as safe (the GRAS list), rather than the academy, because of industrial representation on the academy's committees. Edwards replied, "I think we did . . . I think we did feel that the industry representation on the Drug Research Board (*sic*) was such that it would perhaps be wiser to go with the federation". Edwards was, however, immediately corrected by Virgil Wodica, director of the FDA Division of Foods, who said that FASEB had been chosen so that the academy could be reserved as an appeals body. Edwards had previously said that he believes the academy does a good job in picking the members of review committees, and pointed out that the FDA has difficulty in finding members for its own committees who are completely free from industrial ties.

The charges led Senator Percy to suggest that the academy should draw up a code of ethics for its committee members, although he repeated several times that he has a particularly high regard for the organization. The academy has no official comment to make on the charges, but staff members have pointed out that such a code of ethics has already effectively been established by requiring that each committee member state any possible source of bias, including research support, endowment of his chair and so on.

Other government committees require only a statement from their members of stock held in corporations which may come under their purview.

Moreover, the academy has a report review committee under Dr George Kistiakowsky to look over the reports issued in the academy's name, screening them for possible bias or misrepresentations. They also suggested that it is particularly difficult in the field of nutrition to choose committee members who do not have at least an industrial consultancieship. Unfortunately, Dr Filer, who is now chairman of the Food Protection Committee, could not be reached for his comments.

At least one prominent member of the academy is concerned about vested interests on academy committees, however, for Dr Epstein quoted in his testimony a statement by Dr Harvey Brooks that "It's true that some of our bodies—the Highway Research Board, the Food and Nutrition Board, the Building, Research Advisory Board and the Space Science Board, for instance—may be constituted too completely with those who have an economic or institutional interest in the outcome of their work."

GAMMA-RAY ASTRONOMY

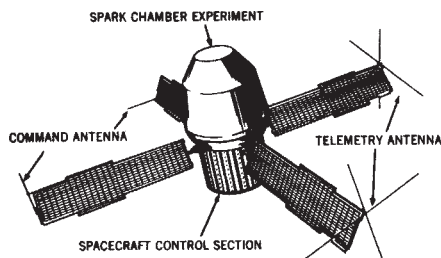
Gearing Up for SAS—B

by our Washington Correspondent

SCIENTISTS and engineers at NASA's Goddard Space Flight Center have now put the finishing touches to a satellite which should open up a new dimension in high energy astronomy. Called SAS-B, the satellite is designed to detect and measure gamma rays, and to locate and study their sources. The satellite was flown to Kenya last week, where it is being prepared for launch, probably in the last week of October, from the Italian San Marco Equatorial Range in the Indian Ocean.

Apart from the small Explorer II, which was flown in 1961, SAS-B is the first satellite devoted exclusively to gamma-ray astronomy. It will be able to detect gamma ray sources more than an order of magnitude below the threshold of any gamma ray detector flown so far, and it will be able to locate the direction of galactic and extragalactic gamma rays to an accuracy of at least 1.5°. Previous detectors, which have mostly been flown on balloons, have picked up gamma rays but have not been able to locate point sources. It is therefore hoped that SAS-B will do for gamma-ray astronomy what its predecessor, SAS-A (re-named Uhuru after launch), has done for X-ray astronomy. Uhuru has now located and studied some 126 X-ray sources; before it was launched, only about 40 were known.

Like Uhuru, the first task of SAS-B will be to survey the sky to look for



SMALL ASTRONOMY SATELLITE - B

and map discrete sources of gamma rays, after which it will be used for a detailed study of individual sources. Astronomers will also be on the lookout for bursts of gamma rays from supernovae, and for pulsed gamma rays from pulsars—it will be interesting to see, for example, whether the rapidly varying X-ray sources discovered by Uhuru are also emitting at gamma ray frequencies. The satellite will be able to pick up gamma radiation in the range 20–300 MeV.

SAS-B will be launched into an equatorial orbit from the San Marco range by a Scout rocket. The orbit will avoid passage through the South Atlantic Ocean magnetic field anomaly, where the electrons and protons would degrade the data from the satellite. The experiment itself, which consists of a digitized spark chamber, will be mounted on top of a standard SAS control section, powered by solar charged batteries and manoeuvred by means of an electromagnet, which is energized by the ground control and which attempts to align with the Earth's magnetic field.

One more launch is scheduled in the SAS (an acronym for Small Astronomy Satellite) series, after which high energy astronomers will focus their attention on the High Energy Astronomy Observatories, which will be flown in the second half of the 1970s, and which will be able to monitor radiation from X-rays to high energy cosmic rays. The launch of SAS-C, another X-ray satellite, is understood to have been put back at least a year, from 1974 to 1975, because of financial stringencies in NASA.

ADVISORY COMMITTEES

Congress Clamps Down

by our Washington Correspondent

EARLY in June, President Nixon issued an executive order designed to make meetings of committees that advise the federal government open to the public. But the order has been limited in its effect because for one thing it specifically excluded those committees that advise the president, and for another it gave considerable discretion to department and agency heads to decide which committees are covered by its provisions. But last month Congress passed legislation which not only attempts to

plug some of the loopholes in the executive order, but also provides a mechanism for getting rid of those committees which have outlived their usefulness. There is some confusion, however, about which committees are covered by the bill because there is a discrepancy between what the bill actually specifies and the interpretation placed on it by the committee which drew it up and it seems clear that the bill will also be limited in its effect.

The bill requires first that the Office of Management and Budget takes stock of the committees that advise the government and provides Congress with an annual statement of their activities and status. Second, each committee in operation when the bill takes effect must be dissolved within two years unless it is specifically reappointed, and similarly, each new committee can be appointed for only two years. Third, within a year after a Presidential advisory committee has reported, the President is required to inform Congress of his proposals for action, or reasons for inaction. And fourth, all advisory committees must be open to the public, unless they deal with matters such as trade secrets which are exempted from the Freedom of Information Act (FIA). In short, the bill specifies that all committees that advise the Federal govern-

ment are subject to the FIA.

The confusion, however, arises from the definition of what actually constitutes an advisory committee. The conference committee which drew up the final version of the bill explained in its report that "the act does not apply to persons or organizations which have contractual relationships with Federal agencies", but the bill itself makes no such exemption. A staff member of the House Committee on Government Operations, which was responsible for drawing up the House version of the bill, explained last week that committees operating under contract are classified as consultant, not advisory, committees. Such an interpretation of the bill would exclude the National Academy of Sciences, which operates under contract to the government, and Chet Hollifield, chairman of the Government Operations Committee, confirmed on the floor of the House that such is indeed the case. Whether the courts would uphold that interpretation of the Act is, however, a moot point, and in any case, it seems to provide a loophole in the bill, for it seems that agencies reluctant to open up their advisory committees to public scrutiny need only call them consultant rather than advisory under contract to bypass the provisions of the bill.

Short Notes

Multiple Sclerosis

During the past year, Congress has launched attacks against cancer, diseases of the heart, lungs and blood vessels, sickle cell anaemia and Cooley's anaemia. It has now turned its attention to multiple sclerosis. Both the Senate and the House of Representatives have eagerly passed legislation setting up a commission to draw up a plan of attack on multiple sclerosis, and although a few details remain to be sorted out by a conference committee, the legislation should have little difficulty in reaching the statute books before the end of the session. The legislation notes that the cause of the disease is unknown, that there is no prevention or cure and that research directed towards combating multiple sclerosis "deserves the highest priority". Consequently, the legislation seeks to establish a commission, composed of six members of the public and five members of the medical community, to draw up a research plan.

HEW Appropriations

The Senate Appropriations Committee is trying to hand President Nixon a hot potato by recommending that Congress sends back to him the \$30,500 million appropriations bill for the

departments of Labor and Health, Education and Welfare, which he has already vetoed. The committee has recommended that Congress sends back the bill with the rider that President Nixon can himself reduce it to the level of \$29,600 million that was passed by the House last month (see *Nature*, **239**, 188; 1972). Such a move would force Mr Nixon to take politically unpalatable decisions over which individual programmes to cut. If the committee's suggestion is accepted by the Senate, it would still have to be accepted by a conference committee to reconcile it with the bill passed by the House last month.

Cancer

The National Cancer Institute has carried out a survey of trends in patient survival after treatment for various forms of cancer. The survey compared 3-year survival rates during the 1940s with 3-year survival rates in the period 1965–69. Marked improvements were found for several types of cancer, including cancer of the bladder, for which the survival rate increased from 48 per cent to 62 per cent, chronic lymphatic leukaemia (33 per cent to 53 per cent), melanoma of the skin (49 per cent to 74 per cent) and cancer of the prostate (49 per cent to 66 per cent). The 3-year survival rate for lung cancer, however, has shown little or no improvement, and stays at about 18 per cent.

NEWS AND VIEWS

Reversed Geomagnetic Events in the Brunhes Epoch

UNTIL 1969 the Brunhes normal geomagnetic polarity epoch seemed to be the only epoch within the well defined polarity scale of the past 4.5 million years which contained no events of opposite polarity. In the sense that reversals of the Earth's dipole field are apparently a largely random phenomenon, there is no *a priori* reason why there should be any reversed events within the 0.69 million years since the Brunhes began, even though the Matuyama, Gauss and Gilbert epochs clearly do include such events. Four years ago, however, Cox (*J. Geophys. Res.*, **73**, 3247; 1968) carried out an analysis of the lengths of geomagnetic polarity intervals using a probability model based on the theory of Bernoulli trials, from which he concluded that the shortest intervals should occur most frequently. In general terms this conclusion was actually supported by the data available at the time in that it was already clear that the frequency of observed intervals decreases with increasing length of interval. But in greater detail, Cox's model suggested that the ratio of shorter to longer intervals should be even higher. Cox thus specifically predicted that additional polarity intervals with lengths between 0 and 50,000 years remained to be discovered within the period of the past 10 million years.

Since that time, experimental evidence—the latest of which is reported by Barbetti and McElhinny on page 327 of this issue of *Nature*—has confirmed Cox's prediction, although this is far from saying that all of the short events within the Brunhes epoch have yet been, or ever will be, discovered. The problem, of course, is the same one which is likely to prevent the extension of the dated polarity-time scale much beyond its present 4.5 million years—that there comes a point where the accuracy of the dating methods employed is not sufficient to resolve the polarity intervals. Beyond 4.5 million years this leads to the impossibility of dating accurately the epoch boundaries; but at more recent times it leads to difficulties in placing events. To this must be added the simple fact of life that the shorter a polarity interval is, the less likely is it to be recorded in the rocks and thus the greater is its chance of remaining undiscovered.

Nevertheless, a few reversals within the Brunhes epoch have been discovered even though their precise positions may remain uncertain. Smith (J. D.) and Foster (*Science*, **163**, 565; 1969), for example, produced evidence from deep ocean sediment cores for a reversed event at about 111,000 years (the so-called Blake event). They overcame the detection problem by concentrating on cores with expanded Upper Pleistocene sections and high sedimentation rates. Palaeontological dating was used, the boundaries of the Blake event thus being estimated at 108,000 and 114,000 years with errors of ± 10 per cent. Stacey (*Physics of the Earth*, Wiley; 1969) has also mentioned the possibility of a reversed event between 330,000 and 350,000 years ago; but the evidence for this, apparently also based on ocean sediment cores, has not yet been published.

The only reversed event of the Brunhes yet found in igneous rocks is the Laschamp event detected by Bonhommet and Zähringer (*Earth Planet. Sci. Lett.*, **6**, 43; 1969) in two lava flows from the Puy de Laschamp

and the Puy de Barme, respectively, in the Chaine des Puys in France. There seems little doubt that these two flows are genuinely magnetized in a reverse direction; but their igneous nature and their geological youth have presented severe dating problems. On the basis of potassium-argon dating of the lava flows themselves, Bonhommet and Zähringer could only say that the Laschamp event ended less than 20,000 years ago. In addition, however, a nearby lava flow, which is clearly stratigraphically younger than the Puy de Laschamp, gave a carbon-14 age of 8,730 years. In other words, if these figures are reliable, the Laschamp event must have ended between 20,000 and 8,730 years ago.

In an attempt to set more stringent limits on the age of the Laschamp event, Denham and Cox (*Earth Planet. Sci. Lett.*, **13**, 181; 1971) made palaeomagnetic measurements on Late Pleistocene sediments deposited in Mono Lake, California. According to carbon-14 dating, the age range of the sediments was 30,400 to 13,300 years ago; and an unusually high sedimentation rate of 4 cm/100 years offered a particularly good opportunity to detect rapid changes in the corresponding geomagnetic field. In the event, no reversals were found—all the sediments were normally magnetized. Unfortunately, there were gaps in the sedimentary record corresponding to time intervals of up to 1,700 years, so it was not possible to rule out completely very short reversed events. By the same token, however, the data enable a boundary condition to be set—namely, that if the Laschamp event occurred between 30,400 and 13,300 years ago, it can have been no longer than 1,700 years.

In fact, as Denham and Cox noted, further conditions can be applied. There are, for example, no archaeological materials known to be reversely magnetized. This sets a lower limit of 6,600 years on the Laschamp event although, as it turns out, this is less stringent than the 8,730 years of Bonhommet and Zähringer. In any case, if the Laschamp event occurred between 13,300 and 8,730 years ago, it cannot have been longer than that period—that is, 4,570 years. At the opposite end of the scale, potassium-argon ages in the range 60,000 to 30,000 years from normally magnetized volcanics from Reunion, Nunivak Island, California and the Galapagos Islands show that the Laschamp event could hardly have been longer than 10,000 years and probably began less than 30,000 years ago.

The only other known reversed event in the Brunhes epoch, apart from that reported by Barbetti and McElhinny in this issue of *Nature*, is the Gothenburg event detected in Swedish varved clay and silt by Möerner *et al.* (*Nature Physical Science*, **234**, 173; 1971) and dated at 12,400 years. In view of what has been said above, it is extremely tempting to equate the Gothenburg and Laschamp events. But this temptation should be resisted for the time being. The only evidence for supposing that the Laschamp event ended less than 20,000 years ago is based on potassium-argon dating, which is unreliable in this age range at the best of times. It is thus just as conceivable that the new event at about 30,000 years reported by Barbetti and McElhinny is the Laschamp event.

In fact, there is an even more likely possibility. The failure of Opdyke *et al.* (*Earth Planet. Sci. Lett.*, **14**, 145; 1972) to detect an event during the past 27,000 years in the Aegean deep sea cores, the similar failure of Creer *et al.* (*Earth Planet. Sci. Lett.*, **14**, 115; 1972) with regard to Lake Windermere sediments of the past 13,000

years, Cox's prediction, and the evidence presented above, combine to suggest that reversals in the Brunhes are many and short.

Thus it is possible that all the events reported so far could easily be different—and but the tip of the iceberg.—From our Geomagnetism Correspondent.

LASA's Window Cleaned?

THREE years ago *Nature* drew attention to the problems of using the Large Aperture Seismic Array (LASA) in Montana for identifying sources at teleseismic distances (222, 413; 1969). Before these comments were made, and again in the intervening period, papers on the structure under LASA appeared with a regularity which gave LASA an unbeatable if unenviable lead in the use of large aperture seismic arrays for studying local structure. Incoherent wave forms, time anomalies as large as 0.5 s, amplitude anomalies, and strong azimuthal variation, all ascribed to local structure, combined to give LASA's source window a rather fly-blown appearance.

This issue of *Nature* (page 318) presents a more sparkling aspect of LASA. The window cleaners are Davies and Sheppard, members of the Lincoln Laboratory group mostly concerned with the use of data from LASA. They have accomplished the task in disarming fashion with the help of an "array diagram" which illustrates earthquake epicentres viewed by LASA in combination with more accurate locations of the global network. The length and direction of the arrows joining the two is a measure of the disturbance in declination ($dT/d\Delta$) and azimuth of the plane wave reaching the array; arrows of zero length represent identity with the standard Jeffreys-Bullen (J-B) tables. So unlike earlier work, both $dT/d\Delta$ and azimuth are used to analyse the causes for inaccuracy in LASA locations.

It turns out that the systematic term in the confusion of arrows is the only deference of the authors to the structural environment of LASA. To be sure, the removal of this term is referred to structure immediately beneath LASA ($dT/d\Delta$ and azimuth errors) and questions concerning deeper lateral variations are now

answered by the argument that the effects would be small, or slow and smoothly varying, for all incoming signals. In this case, the assumptions (J-B tables) used in estimating the global epicentres are no longer valid. More drastic effects are, of course, to be expected from lateral variations in velocity where the seismic rays bottom.

With these arguments accepted, and an intuitive feel for the more plausible explanations, the view of the deep mantle which is thereby revealed is startling to say the least. Geophysicists are now confronted with the possibility of lateral heterogeneity at all depths in the Earth's mantle. To take the most striking example, the array diagram illustrates rapidly varying anomalies wherever Bullen's D' layer is sampled. This area also serves to illustrate the problem of verifying LASA's new look at the Earth. On the one hand, Phinney and Alexander's interpretation is in qualitative accord with the array diagram, while others favour core-mantle or core-core transition layers when interpreting precursors to PKP. If the interpretation of Davies and Sheppard is the correct one, scattering phenomena of the kind forecast by Haddon (*Trans. Amer. Geophys. Union*, **53**, 600; 1972, in abstract) for the boundary between the core and the mantle might be worth looking for. It would also be instructive to attempt a fit of the observed amplitudes to the models proposed by the authors. Modern ray tracing techniques are helping to make this problem more tractable, and an especially interesting test would be of the model which was invoked to explain the observations from the Honshu-Mariana region. This area caused the authors some embarrassment as it is the only source that is generated by the anomaly observed. They may have disarmed critics who

might reasonably ask what criteria they should employ to distinguish deep from upper mantle effects, but a third parameter is badly needed to test intuitive solutions.

It so happens that the only structurally undisturbed view of LASA to the outside world, by way of the Canadian Shield, is the quadrant in which the array diagram displays fewest anomalies. When the arrows are not very small, the azimuths are uniformly correct. A curious coincidence, or a consequence of the siting of LASA? It may take a while before doubts about site effects, which the massive literature on the structure under LASA has implanted, are superseded by the bright new images which LASA presents in this issue.—From a Correspondent.

GENETICS

Sharing the Operator

from our Molecular Genetics Correspondent

THE arginine pathway of *Escherichia coli* comprises one of the more unusual control systems of bacteria, for it consists of nine structural genes located at six different sites on the bacterial chromosome. In spite of their physical separation, however, synthesis of all nine enzymes is repressed when arginine is added to the medium in which the cells are growing. This repression is controlled by a single regulator protein synthesized by a gene unlinked to any of the structural genes. The most important implication of this observation is that the model which Jacob and Monod proposed for the control of the lactose operon, in which all the genes are clustered in one contiguous array, can be extended to situations in which the genes are sited apart.

All the six arginine loci must therefore each have a copy of the same (or a very similar) operator at which the common repressor protein acts. One of these six sites contains four adjacent genes, *argECBH*, and thus appears to be analogous to a conventional operon. But these four genes form two units of expression; *argCBH* constitutes one unit and *argE*, although adjacent, has seemed to be related to them just as the other, unlinked, *arg* genes are, with its own control elements. But in two articles in *Molecular and General Genetics*, Jacoby (**117**, 337; 1972) and Elseviers *et al.* (*ibid.*, 349) confirm previous results but present new data which show that the organization of this locus is unusual.

The grounds for supposing that *argE* is one unit of control and *argCBH* is another are that the enzymes which they

specify are under parallel but not co-ordinate control; *argE* and *argCBH* are repressed by the same conditions but differ in the extent of their response. Another indication is that nonsense mutants which inactivate the *argE* enzyme do not affect the others; but such mutations in *argC* may inhibit the expression of both *argB* and *argH* just as mutants in *argB* may inhibit expression of *argH*. This implies that *argE* is expressed by the transcription and translation of one messenger RNA and *argCBH* by the synthesis and expression of another, reading the genes in the order *argC argB argH*.

Because deletions in the middle of the gene cluster may apparently change the expression of *argH*, it has previously been thought that *argE* is controlled from its left end, so that deletion of the second control elements between *argE* and *argC* places *argH* under the control which usually acts only on *argE*. By selecting cells for their ability to synthesize the enzyme coded by *argC* in conditions in which they can only do so by losing their usual response to the regulator gene, that is by gaining operator mutations, Jacoby has identified the operator locus which governs the *argCBH* cluster. But these cells also have a changed control of *argE*. The operator locus maps between *argE* and *argC*, and has the usual *cis*-dominant control of expression of the structural genes.

An attractive explanation of these results is to suppose that the gene cluster has one operator—which controls both *argE* and *argCBH*—but two promoters, one for each unit. This would explain how both units respond to the same repression, but are expressed to different extents—their promoters must have different affinities for RNA polymerase. Ideas of this sort are borne out by the results of Elseviers *et al.*, whose analysis of deletion strains, taken together with recent demonstrations that the arginine genes are indeed controlled at the level of transcription (Cunin and Glansdorff, *FEBS Lett.*, **18**, 135; 1971; Krzyzek and Rogers, *J. Bact.*, **110**, 945; 1972), suggests that there is but one operator-promoter complex, located between the two genetic units.

A deletion which covers *argC* and *argB* but does not appear to extend into *argE* seems to reduce greatly the extent of synthesis of enzyme from *argE*; this suggests that the elements which control expression of *argE* are located at the right end of the gene, before *argC* which starts the next cluster. In strains with such deletions, *argE* expression can be restored by a mutation which may be located in this region. This supports the idea that *argE* and *argCBH* may be transcribed from opposite strands of DNA in opposing directions, *argE* to the left of a central control site

and *argCBH* to the right. The sequence of sites in the cluster suggested by both articles is *argE*, promoter_{CBA}, operator_{E/CBA}, promoter_E, *argCBH*. By binding to the single operator, the repressor protein could prevent synthesis of RNA from both promoters. This model presents problems of organization during expression, of course, for RNA polymerase may be forced to transcribe both strands in the central region and there may presumably be stereochemical interference between them. More precise mapping of control sites will be needed to determine whether this model is correct, of course, but it may not be unique, for Guha *et al.* (*J. Mol. Biol.*, **56**, 53; 1971) have discovered a similar case of divergent transcription from a central site in the biotin locus of *E. coli*.

When I wrote recently (*Nature*, **239**, 69; 1972) of the advantages of internal promoters for working out the mechanisms of gene expression, only two such elements, located in the histidine operon of *Salmonella typhimurium* and the tryptophan operon of *E. coli*, were commonly known. Deletions in the centre of the *argECBH* locus which delete the control sites seem, however, to allow independent expression of *argH*. This complicates analysis of the locus, but seems to result from the presence of an internal promoter, located either in *argB* or between *argB* and *argH*. This suggests that occurrence of these structures may be common rather than rare.

ASTRONOMY

Cen X-3 a White Dwarf?

by our Cosmology Correspondent

PROPOSERS of the pulsating white dwarf model of pulsars have been hard to find during the past couple of years in

which rotating neutron star models have swept the board. But with the advent of the new age of X-ray astronomy, ushered in by Uhuru, has come evidence to suggest that, even if pulsars are not white dwarfs, other high energy sources may well be represented by these models, updated and with the dust blown off. The best candidate for a pulsating white dwarf now seems to be Cen X-3, which has a period of 4.8 s which can be explained, according to G. Vauclair of Meudon Observatory, in terms of the first harmonic radial pulsation of a 0.6 $M_{\odot}$ white dwarf (*Astrophys. Lett.*, **12**, 17; 1972).

As soon as reports of the 4.8 s periodicity came in last year, several groups were quick to point out that the period found corresponded well with the radial vibration periods of the white dwarf models developed in the early days of pulsar hunting (*Nature Physical Science*, **233**, 18; 1971; and *Astrophys. J.*, **173**, 213; 1972). In that earlier work, however, it was suggested that Cen X-3 might contain a white dwarf of mass roughly 1.1 $M_{\odot}$ vibrating in its fundamental radial pulsation. According to Vauclair, the first harmonic is always more unstable than the fundamental (see also *Astrophys. Lett.*, **9**, 161; 1971), and to explain the 4.8 s periodicity of Cen X-3 in this way would require that the mass of the vibrating star be 0.6 $M_{\odot}$; if Vauclair is correct, the most likely periodic vibration to be excited in a 1.1 $M_{\odot}$ model would be the first overtone 1.6 s oscillation.

An upper limit on the mass of the X-ray variable in the system has recently been derived by van den Heuvel and Heise (*Nature Physical Science*, **239**, 67; 1972; see also *Nature*, **239**, 256; 1972). Using the parameters of the binary orbit they deduce that the mass

Fracture of Polymers

THE standard methods of metallurgical testing can be usefully applied to organic polymer materials to obtain information about their physical properties. Interpretation of the results is, however, more difficult because the more complex structure of a polymer allows energy changes and distributions to be more varied. Studies on polymethyl methacrylate by Johnson and Radon, to be published in *Nature Physical Science* next Monday (October 9), have been concerned with accurate determinations of the fracture toughness by Charpy impact testing.

By monitoring the effects of temperature on the fracture toughness they have shown the effects of changes within the molecular structure of the polymer. The well known β -relaxation point attributed to the ester side groups was prominent, and a novel effect observed was attributed to a transition in the γ -methyl

group. Johnson and Radon calculated activation energies for both these changes.

A reasonable correlation between data derived from electrical techniques, n.m.r. and tensile testing was evident, and the fracture toughness was found to be dependent on the dissipation of relaxation energy.

The results suggest that the fracture toughness of a glassy polymer is independent of strain rate—with the important proviso that the test method should not affect the structure, thermally or mechanically. During fast crack propagation, for example, adiabatic heating was observed. If such effects can be eliminated it seems likely that accurate studies of fracture toughness will allow the detection of changes in the molecular configurations of polymeric materials.

of the star is less than $0.7 M_{\odot}$, and have constructed a corresponding neutron star model of the system. But this upper limit is curiously relevant to Vauclair's discussion of white dwarf models for Cen X-3. On the face of things, it seems at present that the white dwarf models win out on the grounds of simplicity, although without Vauclair's work proponents of the models might have found van den Heuvel and Heise's upper limit uncomfortably restricting.

Although this speculation is interesting because it shows how easily slightly different white dwarf models can be fitted to the observations of Cen X-3, and even to data put forward in support of neutron star models, there must remain some doubt about the value of the exact figures derived in any such calculation. There seems good evidence to support the suggestion that Cen X-3 is an eclipsing binary system in which the white dwarf accretes matter which is lost from its companion, overflowing that star's Roche lobe. In that case, although the energy mechanism for X-ray production is conveniently provided, any detailed calculations based on radial pulsations of non-rotating white dwarf models must be taken as of qualitative value only.

ACETYLCHOLINE

Receptors Resolved

from our Molecular Biology Correspondent
THE acetylcholine receptor has been remorselessly stalked in all quarters of the globe, and in spite, perhaps, of the odd fruitless swish of the butterfly net, several specimens seem to have been captured alive. A modest amount of information has now been compiled about their physical attributes.

For the most part the source of the material has been the electric organ of the electric eel, *Electrophorus*, or of the ray, *Torpedo*. The techniques so far used have involved some form of affinity labelling in the intact cells followed by solubilization with detergent, and fractionation. Amongst recent reports, Meunier *et al.* (*Biochemistry*, **11**, 1200; 1972) used a snake venom α -toxin, into which a tritium label had been introduced, as a marker for the *Electrophorus* receptor, to which it binds with extremely high affinity. On addition of a non-ionic detergent the complex is solubilized in the form of an aggregate of high molecular weight. Such behaviour, in fact, seems to be universal for membrane-embedded receptors, such as those for a variety of hormones. What is more remarkable is that in the presence of the anionic detergent, sodium dodecyl sulphate (SDS), which is a more or less universal denaturing and disaggregating agent, the toxin

seems to remain bound to the receptor, which, as will be seen, is evidently in a monomeric state in these conditions. Electrophoretic mobility in polyacrylamide gel in the presence of this detergent leads to an estimate for the molecular weight of about 55,000, whereas that of the toxin alone is much lower.

Reiter *et al.* (*Proc. US Nat. Acad. Sci.*, **69**, 1168; 1972), on the other hand, obtained a more direct estimate of the molecular weight of the same species based on an affinity-labelling procedure, developed earlier in the same laboratory. The receptor contains a disulphide bond, which is readily reduced, and one of the resulting thiol groups is then highly susceptible to reaction with an acetylcholine analogue, containing the thiol-specific N-benzylmaleimide group. When this reagent is labelled with tritium, a peak of radioactivity emerges from a broad background in SDS-polyacrylamide gels. If the thiol group is protected, by exposure either to the affinity reagent, dithiobischole, which evidently enters the binding site and annihilates the free thiol, or to cobrotoxin, or if an excess of the inhibitor, hexamethonium, is present, the peak is suppressed. Further, because the affinity reagent binds rapidly and specifically, the presence of a moderate concentration of ^{14}C -labelled N-ethylmaleimide does not significantly interfere. This reagent, on the other hand, labels free thiol groups throughout the membrane. Again, against the broad background of label, in an SDS-polyacrylamide gel, a peak of high tritium: ^{14}C ratio stands out, which is lost on treatment with thiobischole. Evidently then the peak of radioactivity in the polyacrylamide gels represents the veritable receptor, covalently labelled.

The molecular weight estimated from the electrophoretic mobility is some 42,000, corresponding, it may be sup-

posed, to the single polypeptide chain. Reiter *et al.* have, moreover, taken a more critical view of the method than others before them: by showing that the molecular weight, relative to a calibration based on standard proteins, does not change with polyacrylamide concentration, they have eliminated, as far as is possible, any likelihood that their protein contains any large quantity of polysaccharide (or lipid), for in such a case less detergent would be bound, and the molecular weight estimate would be in error to an extent that depends on the gel concentration. Bearing in mind the uncertainty in the estimate of the molecular weight of Meunier *et al.*, whose preparative procedure makes it imperative to maintain the receptor in its active state, and therefore precludes the addition of reducing agent to the electrophoresis samples (for otherwise the labelled ligand would dissociate), the agreement (though possibly fortuitous, as the protein is evidently not unfolded) is remarkably good: if one toxin molecule, of molecular weight 7,000, is bound to each receptor chain, the molecular weight of the latter comes out as 48,000.

Eldefrawi and Eldefrawi (*ibid.*, 1776) solubilized the receptor of *Torpedo* with non-ionic detergent, and examined the extract by isoelectric focusing. This is, of course, less informative, for it does not lead to an estimate of molecular weight, and in any case the material is presumably in an aggregated state. Among the multiple protein bands that appear, the receptor activity is confined to a fraction with an isoelectric point of pH 4.5. It seems, however, that in this system phospholipids remain associated with the receptor, for treatment with phospholipase causes loss of activity. The receptor is rather unstable at this pH, and its activity is also diminished by reduction.

Mammalian DNA Repair Endonuclease?

THE so-called excision patch repair mechanism by which microorganisms maintain the integrity of their DNA has been well characterized; it involves three basic steps—the removal of a segment of DNA by an endonuclease, synthesis of a replacement segment by DNA polymerase and sealing of the nicks by ligase. In eukaryotes there is evidence, albeit less complete and compelling, of a similar repair mechanism, and in next Wednesday's *Nature New Biology* (October 11) Brent describes the identification of a putative DNA repair endonuclease in extracts of HeLa cells.

To assay HeLa cell extracts for an endonuclease which preferentially attacks DNA damaged by ultraviolet irradiation, Brent isolated superhelical double stranded circular DNA from the bacteriophage PM2 and irradiated it

with a dose of ultraviolet light estimated to cause radiation damage without actually breaking phosphodiester bonds. He then measured the ability of his HeLa cell extracts to convert irradiated and unirradiated PM2 DNA from the superhelical form to the open circular form.

He found that HeLa extracts would extensively nick irradiated DNA, but that they had little effect on unirradiated DNA and had no exonuclease activity. The endonuclease activity in these extracts is heat labile and is independent of divalent cations, which is also a property of at least one bacterial repair endonuclease. Using normal and xeroderma pigmentosum skin fibroblasts, Brent is now investigating the role of this activity in DNA repair in human cells.

Clark *et al.* (Biochem. Biophys. Res. Commun., **48**, 1061; 1972) have now clarified the apparent differences between *Electrophorus* and *Torpedo* receptors, and the role of the phospholipid. In the membrane both are inhibited, in respect of binding of the specific ligand, α -bungarotoxin, by phospholipase C (and also by trypsin). In non-ionic detergent extracts of *Electrophorus* membranes only, the phospholipase sensitivity falls. This is interpreted to signify that the detergent can replace phospholipid in maintaining the active state of *Electrophorus*, but not of *Torpedo* receptor. In the latter, moreover, the trypsin sensitivity is also much increased on extraction in detergent. A thiol reducing agent diminished the activity of the *Torpedo* receptor in the detergent extract, though not in the intact cell. The ability of the *Electrophorus* receptor to survive when parted from its phospholipid may also be inferred from the remarkable observation of Meunier *et al.* that it will retain α -toxin even in SDS. This can probably be ascribed to the very high association constant.

An interesting parallel is to be found in the work of Kagan *et al.* (Biochemistry, 11, 3412; 1972), who report that the digestion of elastin by elastase is enhanced in the presence of SDS. Free elastase, on the other hand, is denatured by and, after previous incubation with the detergent, will no longer function at all. The elastin certainly is denatured by the SDS. The conclusion is that the enzyme is stabilized towards SDS by its substrate, the binding of which to the native state offsets the stabilization of the denatured form by its association with SDS. This finding may well prove to have wider applications in the field of membrane proteins.

resonance, with no significant sign of splitting. The A1 and the A3 mesons are also better understood, though some ingenuity has been needed to explain the bumps in their mass plots.

Two more mesons have attained respectability. The ρ' is a heavy vector meson which results both from photo-production and from electron-positron

annihilation. Many theoreticians seemed pleased by this, because its existence had been predicted by at least two theories. The other new meson is the S^* , the discovery of which is linked to the consistent looking $\pi\text{-}\pi$ scattering analyses that have recently appeared from Berkeley and CERN.

In weak interactions whole classes of

Aseismic Ridge Dredge Hauls

BECAUSE of the important part played by active oceanic ridges in the new global tectonics, they have received far more attention from Earth scientists in recent years than their aseismic counterparts. One neglected aseismic ridge is the Walvis Ridge, a discontinuous feature trending roughly northeast-southwest in the southeast Atlantic (see map). The northeastern end of the Walvis Ridge seems to be joined to the shelf of Angola and South-west Africa, where Jurassic volcanism produced continental tholeiites and alkali basalts; and the southwestern end is attached to the Mid-Atlantic Ridge through the islands of Gough and Tristan da Cunha which comprise recent suites of alkali-basalt-trachyte. The Walvis Ridge itself is, however, almost completely unknown geologically, the only previous sampling having been Pleistocene to Upper Cretaceous sediment cores and ice-rafted arkosic rocks.

In next Monday's *Nature Physical Science* (October 9), Hekinian remedies this situation to some extent by reporting the results of dredging carried out in 1971 by the RV Jean Charcot. There were two successful geological dredge hauls (CH18-DR3 and CH19-DR4 on the map at about 9° E), although one biological dredge (CH18-CY4) also brought up four fragments of pumice

from the southern flank of the ridge. The rocks from each haul lacked striations and were highly angular and homogeneous, which suggests that they did not have an ice-rafted origin. On the other hand, pillow lava structures could only be recognized on the largest sample recovered.

The rocks from site CH18-DR3 comprised chiefly weathered basalts and pyroclasts. Both types were ferruginous but the basalts were particularly ferruginous, with haematite (Fe_2O_3) contents greater than 10 per cent. Weathered quartz-bearing basalts were recovered from site CH19-DR4. In addition, Hekinian also reports an analysis of a sample from the Walvis Ridge recovered in 1962 by RV Vema at 20° E (V22-146 on the map). This was relatively fresh alkali basalt, particularly notable for its high alumina (Al_2O_3) content of almost 18 per cent.

Some of the volcanics dredged from the Walvis Ridge were highly vesicular, which suggests that they were extruded into relatively shallow water (less than 1,000 m). Unfortunately, however, all samples were too weathered to allow any valid comparisons to be made with adjacent rocks in Angola and South-west Africa, so the relationship between these provinces and the Walvis Ridge remains unknown.

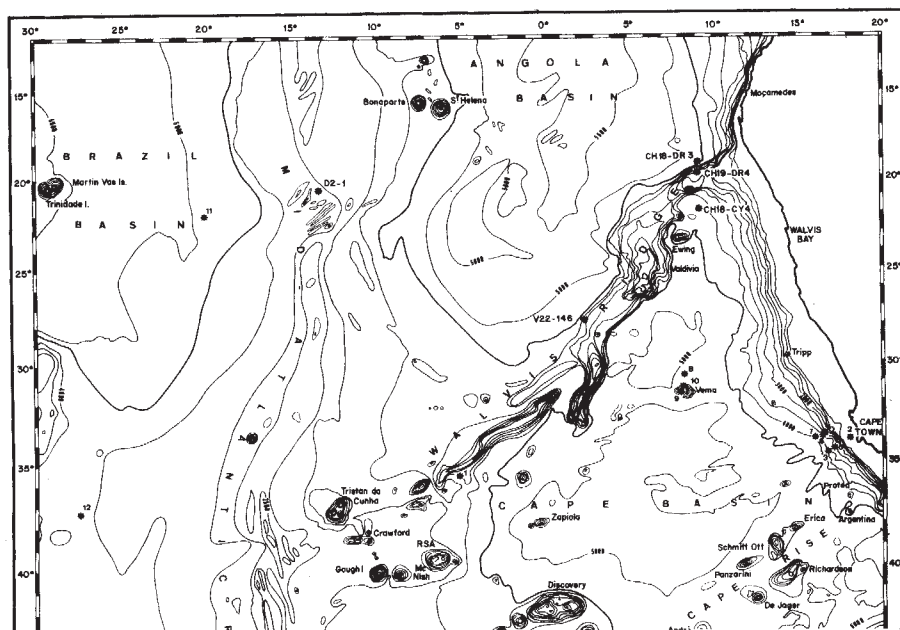
ELEMENTARY PARTICLES

Out with the Old

from a Correspondent

THIS year's "Rochester Conference" on high energy physics was held at Chicago University and at the National Accelerator Laboratory at Batavia, thirty miles from Chicago, from September 6 to 13. Although continuing problems with the machine stopped the laboratory from accelerating protons to 400 GeV during the meeting, members of the laboratory staff were able to provide one unique experience for their guests—they killed one of their herd of buffalo, and gave a buffalo barbecue.

There was no great new breakthrough announced at the meeting, although a number of longstanding problems seem to have been resolved. The A2 meson is now established as a perfectly normal



Map of the aseismic Walvis Ridge.

theories are now in imminent danger of demolition. One class may even have been demolished at the conference—this includes those theories which call for “neutral currents” linked to a massive neutral vector boson. Such a neutral current is expected to give rise to the decay of the K_L^0 meson into a positive and a negative muon. Two experiments were reported which looked for the effect. One of them saw five events, and the claim made is that the rate of decay is one decay into two muons in about 10^8 K_L^0 decays. The other experiments detected no events and claims to set the limit at less than one in about 10^9 . The two groups are now trying to sort out who has the right answer.

Another weak interaction result which has demolished whole classes of theories came from Gargamelle, the large heavy liquid bubble chamber at CERN. The interaction probability for antineutrinos was reported to be approximately one third of the probability for neutrinos at the same energy. This is interpreted as a consequence of the internal structure of the protons and neutrons which are struck by the neutrino beam. In one theory, the result means that only about half of the momentum of a neutron or a proton is carried by the three “valence quarks”.

In spite of the ruling out of some types of theory, the theoretical picture of both weak and strong interactions remains very confused. New data from Serpukhov, from the National Accelerator Laboratory and from the CERN intersecting storage rings have not yet been fitted into a predictive theory of very high energy interactions. Whole classes of weak interaction theories remain, even if the K_L^0 does not go to two muons. The specialists in strong interactions are still divided into many schools, although Professor Murray Gell-Mann (Caltech) is trying to organize a reunion between two of the schools—the makers of “dual models” and those who do current algebra on the light cone.

BIOCHEMISTRY

Replicating DNA

from our Cell Biology Correspondent

WE may not know very much about the biochemistry of DNA replication but, as a glance at the current issue of the *Proceedings of the US National Academy of Sciences* proves, that is not for want of trying, and optimists may justifiably claim that ignorance is being dispelled, albeit very slowly. Barry and Alberts (*Proc. US Nat. Acad. Sci.*, **69**, 2717; 1972), for example, have devised an *in vitro* complementation assay which has enabled them to purify the

products of genes 44 and 62 of phage T4. These two genes with four others, genes 32, 41, 43, and 45, are, as experiments with amber and temperature sensitive mutants have shown, all essential for the replication of T4 DNA. The functions of the products of genes 32 and 43 have, of course, been determined; they specify a DNA unwindase and T4 DNA polymerase. The functions of the other four genes have not yet been elucidated, but Barry and Alberts have now succeeded in purifying the products of genes 44 and 62 as a single electrophoretically homogenous complex of two polypeptide chains with molecular weights of 34,000 daltons and 20,000 daltons; in the complex these two polypeptides have a molar ratio of 4:2. Apparently the complex cannot easily be formed *in vitro* from mixtures of lysates each containing only one of the two polypeptides, but analysis of extracts of cells infected by appropriate mutants indicates that gene 44 specifies the larger polypeptide and gene 62 the smaller.

Using the same *in vitro* complementation assay Hama-Inaba and others in Alberts's laboratory have also purified to homogeneity the product of T4 gene 45. Once the product of gene 41 has been purified Alberts and his colleagues will be in a position to attempt to reconstruct *in vitro* the T4 DNA replication apparatus. They apparently believe that this apparatus may contain all or several of the six polypeptides in some precise macromolecular aggregate and they hope that it may be possible to induce *in vitro* the self assembly of this putative replication complex from mixtures of the six pure proteins in much the same way that Nomura and his associates have achieved the self-

assembly of ribosomes. The belief that the different proteins necessary for the replication of T4 DNA may exist in some precise complex stems in part, of course, from the finding that the gene 44 and gene 62 proteins are complexed together in a manner which survives the abuses of purification. And what Carroll, Neet and Goldthwait (*ibid.*, 2741) have to say about the self-association of pure gene 32 protein lends some support to this idea. Apparently the gene 32 protein forms stable dimers in a variety of conditions including high pH and high ionic strength, and in milder conditions the protein forms decamers and higher aggregates.

Whatever the nature and number of the enzymes involved in DNA replication prove to be, it seems clear that the replication of prokaryotic DNA genomes and some animal virus genomes is a discontinuous process. In other words at least one of the daughter DNA strands is replicated as a series of short DNA chains which are then ligated. Wolfson and Dressler (*ibid.*, 2682) report an analysis of electron-micrographs of partially replicated T7 DNA molecules which leads them to the conclusion that one daughter DNA strand is elongated continuously while the other is synthesized discontinuously. Inman and Schnös and Delius *et al.* have, of course, previously reached the same conclusions from examination of micrographs of partially replicated λ and T4 phage DNAs.

It also now seems generally accepted that an RNA primer is probably required to initiate the replication of *E. coli* DNA and of the DNA of at least some phages. Kornberg's group, for example (Schekman *et al.*, *ibid.*, 2691), report a particularly interesting set of

Cyclic AMP, Cell Surfaces and Mitosis

IN *Nature New Biology* next week (October 11) Burger, Bombik, Breckenridge and Sheppard report a series of measurements of the amount of cyclic AMP in transformed and untransformed cells growing in various conditions, and they present a model of the regulation of cell division which seeks to relate changes in the cell surface to changes in the concentration of cyclic AMP. Burger and his colleagues have, of course, shown that transformed cells are more readily agglutinated by plant lectins than untransformed cells but that, at mitosis, untransformed cells transiently become susceptible to agglutination. And not without some controversy they have suggested that a change in the cell surface, which is at least concomitant with the change that leads to ready agglutination, may trigger a cell to divide. At the same time other groups have reported data which implicate cyclic AMP in the

stimulation of cell multiplication.

Burger and his colleagues therefore measured the concentration of cyclic AMP in synchronized populations of transformed and untransformed mouse fibroblasts; they found that the lowest concentrations are reached during mitosis. Such observations are obviously consistent with the idea that a fall in the concentration of cyclic AMP stimulates a cell to divide. They then asked whether the round of cell division which follows the exposure of mouse fibroblasts to proteases can be blocked by adding 5×10^{-6} M dibutyl cyclic AMP to the medium. It turned out that as long as the cyclic AMP is added within 3 min of exposure to the protease the cells do not divide. They speculate, therefore, that changes in the cell surface may, by some unspecified mechanism, cause the concentration of cyclic AMP to fall and result in a round of cell division.

experiments which show that the conversion *in vitro* of single-stranded ϕ X174 DNA to the double-stranded replicative form depends on the presence of the four ribonucleoside triphosphates, is inhibited by small amounts of actinomycin D and involves the covalent linkage of DNA to an RNA primer. The conversion is not, however, inhibited by rifampicin or by streptolydigin, both antibiotics which not only block RNA polymerase but also prevent the initiation of replication of the single-stranded DNA of coliphage M13. Precisely what enzyme, or what form of the classic RNA polymerase of *E. coli* synthesizes the RNA primer involved in initiating the replication of ϕ X174 DNA, even in the presence of these two antibiotics, remains an intriguing question. The roles in the replication of the DNA of these two phages of the products specified by the genes *dnaA* and *dnaB* of the host cell also pose an interesting and unsolved question. Kornberg's group suggests that both *dnaA* and *dnaB* gene products are required for the initiation of replication of ϕ X174 DNA, but are not required for the replication of M13 DNA. The experiments reported by Hofschneider and his colleagues (*ibid.*, 2570) suggest, however, that a functional *dnaB* gene may be required for replication of M13 DNA.

SPACE MEDICINE

Acclimatized Rats

from our Soviet Correspondent

EXPERIMENTS carried out in the Tuya-Ashu pass in the Tien Shan Mountains indicate that, following acclimatization at a little more than 3,000 m above sea level, the altitude ceiling of resistance to hypoxia may be increased by more than 1,000 m to some 15,000 m (*Kosmicheskaya Biologiya i Meditsina*, 6, No. 4; 1972).

The experiments were carried out on albino rats weighing between 160 and 180 g. Control experiments were carried out in Frunze (altitude 771 m). Two series of tests were performed: in the first, six groups of ten animals were used and the altitude ceiling was established on the third, seventh, fifteenth, thirtieth, forty-fifth and sixtieth days of acclimatization; in the second series, four groups of ten rats were kept for fifteen, thirty, forty-five and sixty days at the Tuya-Ashu station, and then returned to Frunze, where they were tested for resistance to hypoxia on the third, tenth, twentieth, thirtieth and fortieth days after return. All tests were carried out under identical temperature conditions (18–20°C), and the "ascent" was simulated in a pressure chamber at a rate of 16.6 m s⁻¹ with a pause of 5 min after each additional 1,000 m of "height". The test was terminated immediately respiration ceased.

The control experiments in Frunze gave an altitude ceiling of 13,700±324 m. Following transfer to Tuya-Ashu, the altitude ceiling at first decreased (to 13,250±397 m on the third day) and then increased, the maximum resistance to hypoxia being observed on the forty-fifth and sixtieth days (altitude ceilings of 15,200±265 m and 15,000±397 m respectively). The results for the thirtieth day (14,750±397 m) indicate, however, that by this time the physiological changes involved in acclimatization had taken place. On return to lower altitudes, the duration of the increased resistance depended on the period of acclimatization; the effect of acclimatization for 45 to 60 days lasts for more than 2 months, whereas that of acclimatization for 15 to 30 days lasts only for 20 to 30 days and by the fortieth day has decreased to virtually that of the control experiment.

MOLECULAR BIOLOGY

More Uses for Neutrons

from a Correspondent

A NEW method involving neutron scattering has been proposed for the determination of biological quaternary structure (Engleman, D. M., and Moore, P. B., *Proc. US Nat. Acad. Sci.*, 69, 1997; 1972).

It is about ten years since the three-dimensional arrangement of a protein molecule was first determined; now the number of such known structures is counted by the dozen and interest centres not so much on the structure

itself as on the biological mechanisms which it might explain. These molecular structures could be determined because the protein molecules crystallized, that is arranged themselves on a three-dimensional lattice. It was then possible, by the techniques of X-ray crystallography, to determine the structure of the occupants of the lattice points. Interest is now growing in the question of how these biological macromolecules, such as proteins, interact with one another in ordered aggregates (quaternary structure) to produce biological systems such as ribosomes, membranes and muscle. Such large scale aggregates do not always crystallize readily, and it is obvious that unless these biological systems are themselves to some extent crystalline—fortunately various animal frameworks and muscle are—it is not possible to apply diffraction techniques so readily, and the task of determining molecular arrangement *in vivo* can become very difficult.

The new method depends on the physical basis for neutron scattering. X-rays are scattered by the electron clouds surrounding the atomic nuclei, and so the result of a structure determination by X-ray crystallography is a contour map of electron density. An important method for solving molecular structures by X-ray crystallography is to introduce a heavy atom into the molecule; the large number of electrons associated with this atom then dominate the X-ray scattering and the position of the atom is readily found. Neutrons, on the other hand, "see" the atomic nuclei, and neutron scattering is not simply related to atomic number.

Inside Cool Interstellar Clouds

FROM an examination of the radio frequency recombination lines in the direction of the interstellar cloud W3A it is possible to make some conclusions about conditions in interstellar space. This study is reported in next Monday's *Nature Physical Science* (October 9) by E. J. Chaisson of Harvard College Observatory. Chaisson has used the 140-foot aerial of the National Radio Astronomy Observatory to examine the 18 cm signal from the direction of W3A. The interesting features that Chaisson describes are a number of recombination lines which are superimposed on the continuum radiation emitted by the HII region associated with W3A. The recombination lines are presumed to arise from cooler, partially ionized concentrations situated somewhere along the line of sight between W3A and the Earth.

In his article Chaisson reports the detection of recombination lines of excited hydrogen and carbon, and a line attributed to so far unidentified elements

of intermediate mass. By examining the widths of the lines he can, of course, obtain some measures of the temperatures and amount of turbulence within the cloud. Chaisson then goes on to discuss what conditions in the clouds are likely to allow the coexistence of the *mélange* of radio frequency lines which are observed in that direction. One possibility is that the outer parts of the cloud shield the inner regions so that conditions are suitable for the existence of a variety of molecules and ionized atoms. And Chaisson speculates that if the line attributed to the superposition of emission from several unidentified elements can be disentangled, the line might give valuable information about the relative abundances of the elements in these cool clouds. But this possibility seems to depend more on the amount of turbulence within the clouds than on questions of instrumental resolution. It may be that the only differentiation that can be made will be the separation of iron from other species.

The application of neutron scattering to molecular biology has been pioneered by B. Schoenborn at the Brookhaven National Laboratory, who has used the method to show up the positions of hydrogen atoms in crystalline myoglobin. Because of their low atomic number hydrogen atoms are virtually invisible to X-rays, but they scatter neutrons strongly and negatively. The method has been extended by Caspar and Krishner so that it can be used to define the location of water in myelin sheath. This involves the exchange of D_2O for H_2O and makes use of the fact that the scattering lengths of D and H are very different.

Engleman and Moore once more make use of this scattering difference between D and H. Ribosomes are complex aggregates of a number of protein molecules and their subunits are themselves macromolecular aggregates and can be reconstituted from their components. Now if a subunit could be reconstituted with two of the components rich in H while the rest of the subunit was heavily deuterated, then it can be shown that the vector relating the two components rich in H would be easy to determine. The authors then point out that if the relative positions of the components rich in H could be varied in a controlled fashion by the reconstitution process, then vectors between all possible pairs of component could be highlighted successively; the amount of information is then more than sufficient to solve the quaternary structure and this can be done by a triangulation method. Engleman and Moore present calculations to illustrate the feasibility of the method, and they reveal that it yields a contrast equivalent to an X-ray experiment with a uranium atom for every three amino-acids.

SEMICONDUCTORS

Acoustic Waves

from a Correspondent

THE annual meeting on semiconductor devices sponsored by the Institute of Physics was this year incorporated in the second European Solid State Devices Research Conference, and was held at the University of Lancaster from September 12 to 15. Elevation to the status of a European conference was reflected in the increased attendance and number of papers submitted.

Of the invited speakers reading review papers, undoubtedly the greatest interest for many of the delegates was the paper by Professor W. Shockley (Stanford University, California) who described his work at Bell Telephone Laboratories in the late 1940s. This work led to the development of the transistor and constituted one of the principal landmarks

in the history of electronics. Although rather historical in content, Professor Shockley's lecture gave an insight into the problems and obstacles encountered on the road to an important discovery.

Returning to more recent events, the conference papers as a whole echoed the ever-changing emphasis in the field of semiconductor devices. The increasing interest in surface acoustic waves during the past few years was reflected in the devotion of a full session to the topic. After an introductory review by Dr E. G. S. Paige (Royal Radar Establishment, Malvern), a succession of papers described several electro-acoustic devices. Piezoelectric ceramic materials seem to be finding increasing applications in this field, for example in surface wave delay lines in which the delay time can be varied. Dr F. G. Marshall (Royal Radar Establishment) described a new multistrip device which couples two independent surface waves. This device allows the vector sum or difference of the two signals to be obtained, and may be used to halve the width of a beam or to double the effective intensity.

Transferred electron devices, Gunn and Impatt diodes, occupied much of the proceedings. The theoretical models describing the Gunn effect in certain materials are the subject of some controversy, and this was evident in the papers presented. Support for the so-called three-level model was provided by Mr B. A. Prew (Royal Radar Establishment) who reported experimental measurements of the electric field distribution of dipole domains propagating in indium phosphide. Some interest was also apparent in contact effects in Gunn devices.

Dr H. C. Card (University of Manchester Institute of Science and Technology) described his work on the determination of the population of interface states in devices that involve the tunnelling of electrons through thin insulating layers. Such devices include electroluminescent metal-insulator-semiconductor tunnel junctions, and the subject of luminescence was the basis of a number of papers. The Royal Radar Establishment is again in the forefront in this field and a paper by Mr A. L. Mears and his colleagues described the high brightness which can be obtained with zinc sulphide electroluminescent panels. The problems associated with many-character alphanumeric displays were discussed, and it was shown that long display lifetimes may be obtained by the use of pulse techniques.

An alternative display medium is afforded by using liquid crystals. Mr R. C. Bottomley (SERL, Baldock) discussed an attempt to overcome the resolution problems normally encountered in liquid crystal displays when addressing methods using electron beams are

employed. By using a "write gun" selected areas of the target can be activated by secondary electrons, the resulting storage display giving both high resolution and high illumination levels.

A notable advance in the operation of GaAs lasers was described by Dr G. H. B. Thompson (STL, Harlow). He showed that by narrowing the carrier confinement region in a double heterostructure laser to a width of $0.18 \mu\text{m}$ a threshold current density of only 820 A cm^{-2} could be obtained. The multilayer structure of these lasers poses a number of fabrication problems, but once overcome threshold current densities for lasing action of as low as 300 A cm^{-2} are predicted.

RELATIVITY

Apollo Clock Corrections

by our Cosmology Correspondent

ALTHOUGH widespread interest in the relativistic time shift suffered by a spacecraft has grown up as a result of the Apollo missions to the Moon, until recently calculations of the magnitude of such an effect were entirely hypothetical. Now, however, John Lavery, of the Goddard Space Flight Center, has published the results of precise calculations using actual tracking data for the Apollo 12 and 13 missions (NASA Technical Note, D-6681, NASA, Washington, 1972). These calculations confirm that it is within the state of the art today to measure these effects.

The principal factor causing the difference in time elapsed as recorded on a clock carried around the Moon compared with one which remains on Earth is that the moving clock is carried through a part of the Earth's gravitational field in which there is a steep gradient. The problem is not, except incidentally, concerned with the time dilation due to motion of Special Relativity, but with the gravitational time shift of General Relativity. It is the influence of the Earth's gravitational field which predominates, because in the region of the Earth-Moon system the Sun's gravitational field is roughly constant and, of course, the Moon itself only has a very small influence compared with the Earth, because of its much smaller mass.

According to Lavery's calculations, the total time advance of a hypothetical standard clock on Apollo 12 would have been $570.3 \pm 0.3 \mu\text{s}$ and for a similar clock on Apollo 13 the advance would have been $327.6 \pm 0.2 \mu\text{s}$. These effects are certainly measurable using modern caesium or rubidium clocks but, unfortunately, it seems unlikely that an appropriate experiment can be accommodated on the sole remaining Apollo mission.

Two Hundred Years of the Radcliffe Observatory

A. D. THACKERAY

Radcliffe Observatory, Pretoria, South Africa

Dr Thackeray, the Radcliffe Observer, traces the history of the Radcliffe Observatory at Oxford and later in South Africa.

ON June 27, 1972, the Radcliffe Trustees celebrated the bicentenary of the laying of the foundation stone of the Radcliffe Observatory in Oxford. This observatory was the second to be established in Britain as a permanent institution. The Savilian Professor of Astronomy, the Rev Thomas Hornsby (1733–1810), had successfully applied to the trustees of Dr John Radcliffe (who died in 1714) for funds to be allocated for this purpose after completion of the Radcliffe Infirmary.

The Royal Observatory had been built at Greenwich in the previous century in order to assist navigation. The Radcliffe Observatory under Hornsby was similarly devoted to securing accurate positions of Sun, Moon, planets and stars. But although such observations would also be useful to navigation, the interests of astronomers of Hornsby's time and later were chiefly devoted to studying refined aspects of planetary motions to test Newton's gravitational predictions and to the growing topic of proper motions of the "fixed stars" (first detected by Halley in 1718). Observatories with similar objectives sprang up in the British Isles—at Dunsink (1785), Armagh (1790), Edinburgh (1811) and Cambridge (1823)—and elsewhere.

The original equipment of the Radcliffe Observatory consisted of an 8-foot transit, two 8-foot mural quadrants, a 12-foot zenith sector (little used) and an equatorial sector, probably the best instruments made by the celebrated John Bird. It was generally recognized that in its earliest period under Hornsby the Radcliffe Observatory was one of the best-equipped, if not the best, observatories in the world. From 1774 to 1803 when Hornsby stopped observing at the age of seventy owing to ill-health, this remarkable observer had recorded 80,000 transits and 20,000 zenith distances. Hornsby worked single handed even though his usual practice was to observe the same star with transit and mural quadrant during a single transit. It is now recognized that the accuracy of his observations was exceptionally high—in Right Ascension his measures had errors about 40% those of his predecessor Bradley (who is generally regarded as the pioneer of accuracy in astronomical observation) and even somewhat less than those made at Greenwich in 1852 under Airy.

As examples of Hornsby's accuracy and realistic treatment of observations by others, he derived from transits of Venus a solar parallax of 8.78 arc s (to be compared with 8.79415 arc s (now), although Encke's value of 8.57 arc s was accepted for many years following 1824. Moreover it came to light during this century that Hornsby's observations of Sirius show

the orbital motion (announced forty years later by Bessel) caused by the pull of the white dwarf companion. Hornsby narrowly missed the discovery of Castor's binary motion when he pointed out the common proper motion of the two components.

Hornsby's observations are still of value, in spite of the relatively high accuracy of modern observations, because of the long time-span. They are more consistent with the best determinations today of the correction to Newcomb's precessional constant than any other early work. It is this precessional constant which directly affects systems of proper motion catalogues and hence lies at the root of the distance scale within the Galaxy and of other galactic constants. Early observations like those of Hornsby contribute towards problems of the rate of advance of the perihelion of Mercury, explained by relativity, and of the irregular rotation of the Earth.

Hornsby began observing in 1774 long before the historic building next to the Radcliffe Infirmary was complete. He kept a careful watch on possible influences of the building process on his instruments. Although in essentials the building was complete by 1778, the final touches to the octagonal tower, copied from the Temple of the Winds in Athens, and to the interior, were not completed until 1793.

From Hornsby's death (in 1810) to 1839, the observatory continued this same positional work under the next two Savilian Professors of Astronomy, Abraham Robertson (1751–1826) and Stephen Rigaud (1774–1839). These two observers recorded 53,000 transits and 41,000 zenith distances, but unlike Hornsby they employed assistants. These observations have never been reduced or published and might repay study.

In 1839 an unfortunate split occurred between university and trustees. The university appointed the Rev G. H. S. Johnson as their Professor of Astronomy, but the trustees, under the chairmanship of Sir Robert Peel, appointed a much more practical astronomer, Manuel Johnson. Consequently the university had to wait another twenty-five years before a second observatory was built in Oxford for the Savilian Professor.

Manuel Johnson had made his name as an astronomer on St Helena. He compiled a catalogue of positions of 660 southern stars for the East India Company, the earliest southern material that is still in use in systems of proper motion. Johnson detected the large proper motion of α Centauri and imparted this information to Henderson who later reaped the honours for one of the first measures of a stellar parallax after he had examined his own measures made at the Cape of Good Hope.

At Oxford, Johnson started the regular annual publication of observations and the compilation of the first of a series of six Radcliffe Catalogues of stellar positions. Following Bessel's successful measure of the parallax of 61 Cygni with a heliometer, the Radcliffe Trustees acquired for the observatory the only heliometer ever to be used in England. Johnson used it for studies of variable stars, and attempted to measure trigono-

metrical parallaxes with it before it was realized how few bright stars were near enough to be within the range of its capabilities.

One of Johnson's assistants was Norman Pogson who is remembered for his precise definition of the term "stellar magnitude". An exceedingly active observer, while at Oxford he discovered four minor planets.

Johnson died in 1859. His two successors Robert Main (Radcliffe Observer, 1859–1878) and E. W. Stone (1879–1897) carried on the traditional work of the observatory embodied later in *Radcliffe Catalogues*. Stone was especially interested in the problem of the solar parallax and used the heliometer for minor planet observations in attempts to improve this fundamental quantity.

A. A. Rambaut, who succeeded Stone in 1897, was anxious to acquire a large refractor for stellar photography which had scarcely begun in Britain. The trustees approved the purchase of a twin 24-inch (photographic) and 18-inch (visual) refractor. Thus at the turn of the century the Radcliffe Observatory abandoned its meridian work which had occupied it for 130 years. Attention was at first directed to photographic stellar

through some petty obstruction. In the 1920s, with the warm support of the Astronomer Royal (Sir Frank Dyson) a Royal Society grant, eventually totalling £900, was obtained together with the gratuitous assistance of Jackson then chief assistant at Greenwich. It was this reduction of Hornsby's early work that provided the final proof of this remarkable observer's accuracy as quoted above.

Move to South Africa

In the 1920s it was already clear that observational astronomy in Britain had fallen far behind that in the United States because of the poor climate and lack of large telescopes. Knox-Shaw, with experience of better conditions in Egypt, was anxious to move while the Oxford Medical School in the Infirmary next door was anxious to expand. Lord Nuffield's munificent donations to medical research eventually led the trustees to sell the Radcliffe Observatory and land for £100,000 out of which £65,000 was set aside for the building of the new Radcliffe Observatory in South Africa, incorporating a 74-inch (1.88 m) reflector by Grubb-Parsons. The Pretoria Municipality



Fig. 1 The Radcliffe Observatory, Pretoria.

parallaxes but later diverted more profitably to proper motions as an important part of Kapteyn's international plan of Selected Areas. First-epoch plates were taken and stored by Rambaut; second-epoch plates were taken by his successor Harold Knox-Shaw who began service as Radcliffe Observer in 1924. Knox-Shaw and his assistants had to struggle hard through ten years to complete the observing in deteriorating conditions and to measure and reduce 32,000 stars down to 15 mag. The *Radcliffe Catalogue of Proper Motions* was published in 1934. This ambitious work was unique in its coverage of the sky down to such a faint limit. Observations have been repeated in a few selected areas with the same telescope (now at the University of London Observatory) and reductions are in progress at Royal Greenwich Observatory and at the Kapteyn Laboratory (Groningen).

Another feature of Knox-Shaw's service in Oxford was the publication by Knox-Shaw, Jackson (of Greenwich) and Robinson of Hornsby's *Meridian Observations 1774–1798*. Stone and Rambaut had both recognized the value of Hornsby's unpublished work but neither had succeeded in obtaining funds for their reduction; Rambaut in particular only failed to do so

generously donated a site of 57 acres on the Klapperkop ridge $4\frac{1}{2}$ miles south-east of the city, and laid on water and electricity.

Unfortunately the scheme was opposed by the University of Oxford, chiefly by Professor F. A. Lindemann (Lord Cherwell) on the legal grounds that there was no precedent for such a large sum of Charitable Trust money being spent outside the jurisdiction of the Court. The trustees won the case after a delay of more than two years. Further delays arose owing to two failures in casting the 76-inch disk in the United States. But for these delays and the intervention of the Second World War there is little doubt that the observatory (Fig. 1) would have been operating the largest telescope in the southern hemisphere by 1936 instead of by 1948, the date of its final commissioning. At that date, with Knox-Shaw about to retire, there were only two assistant astronomers and three African labourers, and no mechanic or secretary.

In 1951 it became practicable to expand the staff and to use all available dark hours. An agreement was drawn up between the trustees and the Admiralty (as controllers of the Royal Observatory, Cape). Two Cape astronomers were allotted one

night in three and in return the observatory received an Admiralty grant of £2.5 thousand, later increased to £3.5 thousand a year. In 1958 the Astronomer Royal, Sir Richard Woolley, secured a DSIR grant which permitted instrumental improvements; British astronomers also visited the observatory in considerable numbers, supported by DSIR grants. In 1967 the Science Research Council entered into a seven year agreement with the trustees whereby the council took over administrative and financial control while the trustees continued to make a contribution out of their Astronomy Fund.

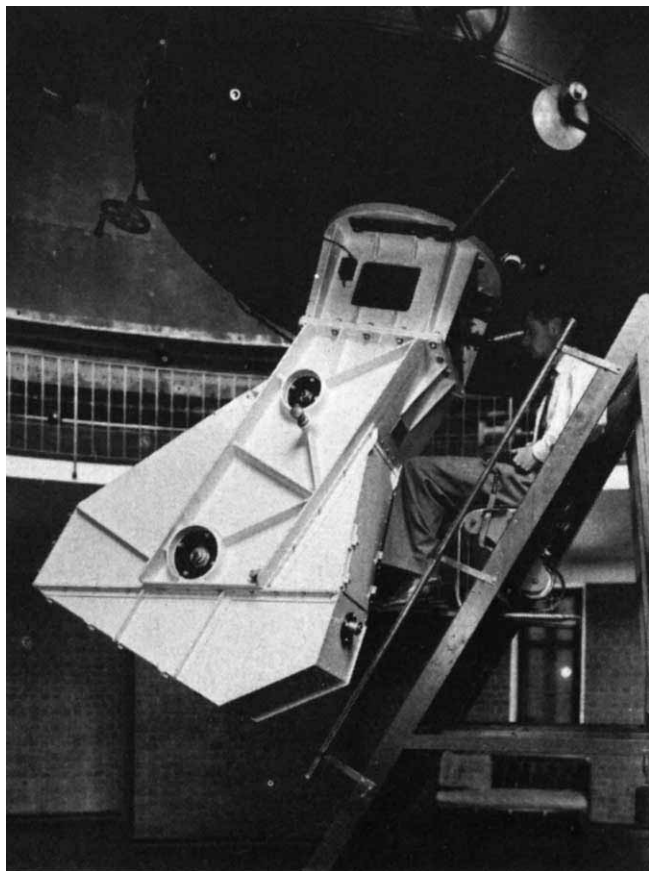


Fig 2. The two-prism spectrograph.

From 1948 to 1968 the Radcliffe reflector was the largest British optical telescope and it is still the equal largest in the southern hemisphere with unique opportunities for studying the Magellanic Clouds, the galactic nucleus, the southern Milky Way and numerous other objects inaccessible to northern astronomers. The project for which the move from Oxford received strong international support was the extension of the work of the Dominion Astrophysical Observatory, Victoria, BC, to the southern hemisphere. About 1930 J. S. Plaskett and J. A. Pearce demonstrated the differential rotation of the Milky Way through measures of radial velocities of distant B-type stars. Their analysis was weakened by the distorting effect of the local streaming motion of the Scorpio-Centaurus association in the south. Work on radial velocities in the south practically stopped in 1930. When it was resumed at the Radcliffe Observatory in 1951 the observation of fainter B-type stars beyond the Scorpio-Centaurus stream was the first priority. The data were supplied by Feast, Thackeray and Wesselink who used a first-class two-prism spectrograph (Fig. 2) designed chiefly by R. O. Redman with optical parts by Adam Hilger and mechanical parts by Casella. Supplementary data on cepheid variables, whose distances are probably more accurately known, were

provided by the Radcliffe Fellow, D. W. N. Stibbs. The observations yielded refined values of galactic constants just at the time when they were most needed. In 1951 the radio astronomical measures of the hydrogen 21 cm line first began to penetrate to the bounds of the rotating spiral galaxy. Spiral arms could, however, only be mapped on the basis of a model whose constants had to be supplied in part by optical astronomers. The distance to the galactic centre, still subject to controversy, was found from Radcliffe material to be of the order of 9 kpc. Other investigations of galactic dynamics have also occupied much observing time. For instance, Cape astronomers concentrated on velocities of nearby stars. T. D. Kinman, another Radcliffe Fellow, measured velocities of old objects—the globular clusters and RR Lyrae variables, while Feast studied the long period variables which appear to cover a wide span of ages. Feast and Thackeray found the globular cluster 47 Tucanae to be surprisingly metal-rich at a time when most globular clusters were considered to be old and metal-poor.

In the meantime the Magellanic Clouds, the two nearest external systems, were intensively observed as perhaps the most important southern objects. In 1951 the first RR Lyrae variables were found in an external system, 1.5 mag fainter than expected. This provided immediate confirmation of Baade's revised distance scale (1952) which was deduced partly because the RR Lyrae variables in M 31 were too faint even for the 200-inch telescope. The spectra of Magellanic Cloud stars had hardly been studied at all when the Radcliffe reflector began work. 150 bright members were observed and shown to be very similar in general composition to supergiants in the Galaxy, but some unexpected red super-supergiants were discovered and their evolutionary status is still uncertain. Radial velocities of stars of various types have given considerable insight into the dynamics of both Clouds including a demonstration of the rotation of the Large Cloud.

As with all modern reflectors many objects of special astrophysical interest have been studied—too many to be summarized here. Of outstanding interest is Eta Carinae which shows some similarity to quasars. The expanding halo around it, apparently the remnant of the 1841 outburst, was shown at Radcliffe to be strongly polarized and its redshift (running up to about $+900 \text{ km s}^{-1}$) is probably due in part to scattering by the expanding shell rather than to recession. The spectrum is rich in forbidden lines some of which have been seen nowhere else. The slow nova RR Telescopii similarly has shown unique lines due to [Fe IV], [Ni IV], [Ca VII] and so on, which aid analysis of the energy levels of these ions.

In the twenty-four years since 1948 the Radcliffe reflector has been used by nearly 100 astronomers, mostly from Britain, but also from Australia, Canada, Eire, France, Holland, Italy, South Africa and the United States. Averaging more than 2,400 h a year (more than the Californian telescopes) it has probably been used for more hours than the telescopes in 160 years at Oxford, where work usually ceased at midnight. It is astonishing that the initial equipment which contributes so heavily to British observational astronomy was paid for out of funds dating from 1714. With the generous support of the Science Research Council costly modern equipment, like the image tube spectrograph constructed at Royal Greenwich Observatory, has now been installed.

It is not surprising that the trustees, having had to depend so much on external sources to finance the observatory, are now seeking to dispose of it. The Science Research Council, with its heavy commitments towards the Anglo-Australian 150 inch reflector, is not favourably disposed towards continuing its support as at present once that telescope is operating. The move of the Radcliffe Observatory from Oxford has been amply justified by the products of two decades of research carried out on a small budget. It will be a far harder task for the rising generation of young British astronomers to justify their more costly share of the new astronomical equipment being set up elsewhere.

Error Theory and Ageing in Human Diploid Fibroblasts

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Immunological and enzymatic assessments of lactate dehydrogenase in human lung fibroblasts strongly suggest that altered proteins accumulate in ageing cells. This supports but does not prove the error catastrophe theory of all death.

DIPLOID fibroblasts of human^{1,2}, chick embryo^{3,4} and bovine⁵ origin have a limited growth potential *in vitro* and are therefore being used increasingly as a model for studying ageing⁶, but whilst considerable data support the validity of the system^{6,7} there is no conclusive evidence for any of the current theories of ageing. Loss of mitotic potential⁸ and the ultimate death of these cells could arise either from an accumulation of faults in informational macromolecules or a genetic programme related to differentiation; but although experiments have been designed to distinguish between these two possibilities⁹ the results can usually be interpreted in favour of either theory. Various changes occur during the life-span of human fibroblasts in culture¹⁰⁻¹³ and we have now attempted to determine whether the reduction in enzyme activity in ageing cells is the result of increasing mistranslation¹⁴. It has already been shown that progressive mistranslation is associated with death in some fungal strains^{15,16} and although it has been suggested that the same is true for diploid fibroblasts¹⁷ further evidence is necessary before Orgel's error theory might be generally accepted.

If mistranslation is associated with ageing in diploid fibroblasts we might expect to find that age-associated loss of enzyme activity¹³ is the result of dilution by inactive enzyme molecules. We have therefore chosen to examine the amounts of active and inactive lactic dehydrogenase (LDH) in cells of various ages using the human diploid fibroblast line, MRC-5, which is derived from human embryonic lung¹⁸. The amount of enzyme has been assessed both immunologically and enzymatically so that the quantity of enzyme can be expressed in two ways; in units of antigenic activity (cross-reacting material, CRM) and in units of catalytic activity^{16,19}. Any disparity between these two values with an increase in the relative proportion of antigenic activity would indicate that catalytically inactive molecules were present. After assaying the amount of enzyme in crude extracts of cells²⁰ the amount of CRM was assessed by titration with antisera and the amount of antisera which was theoretically required for total inactivation was taken to be a measure of the total amount of LDH present. Fig. 1 shows the results of these determinations: the progressive loss in specific activity of LDH is clearly not associated with a loss

of the enzyme protein. This might well be predicted on the basis of the error theory and suggests that inactive protein is accumulating in the ageing cells. There are, however, other possible explanations.

Singh and Kanugo²¹ have shown that the relative proportion of LDH isozymes in various tissues of rats changes as the animal ages and although this may have a physiological explanation^{22,23} similar changes in ageing fibroblasts might interfere with the immunological assay. Fig. 2 shows that the isozyme distributions in samples of differently aged cells do not alter. Indeed, densitometric analysis shows that the muscle to heart-type isozymes are maintained in a ratio of 0 : 1 : 5 : 8 : 2 throughout the life-span of the cells, so changes in the isozyme distribution seem unlikely to be the cause of the results shown in Fig. 1.

But we still cannot exclude the possibility that "worn out" enzyme molecules accumulate in an inactive form once the cells have already died. The apparent exponential reduction in the proportion of active enzyme would then merely reflect the numbers of dead or dying cells in an ageing culture.

We have therefore adopted another technique, involving assessment of the ability of a cell to distinguish between an amino-acid and its chemical analogue²⁶. MRC-5 cells were found to be capable of discrimination against ethionine when this ¹⁴C labelled amino-acid analogue was supplied in an equal proportion with that of ³H methionine, but although the amount incorporated into protein was about 50 : 1 (methionine: ethionine) in young cells, this discriminatory capacity was almost completely lost in aged cells (Fig. 3).

This strongly suggests that errors in the protein synthetic machinery are increasing with age but once again there is an alternative explanation. A reduction in the total number of

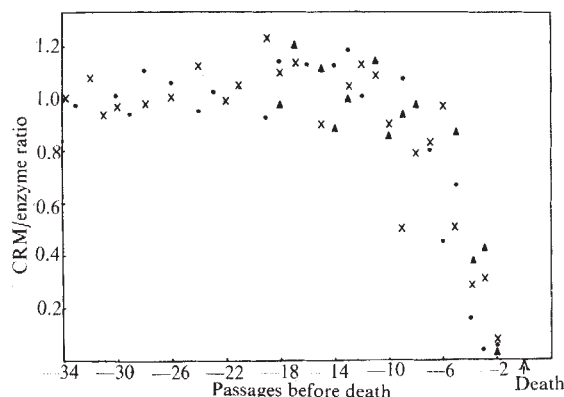


Fig. 1 The change with age in the ratio of catalytically active enzyme²⁰ and enzyme protein detected immunologically^{16,19} with antisera raised in New Zealand white rabbits. The results are those obtained from three independently maintained cultures in which the loss of mitotic potential occurred at passes 52, 58 and 60.

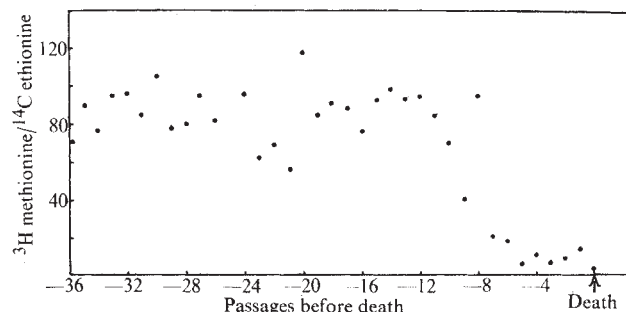
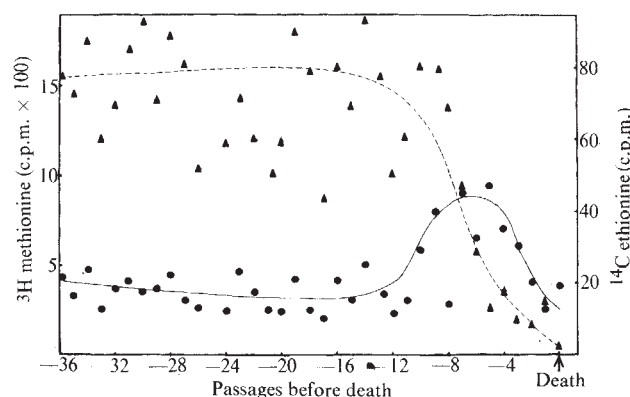
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Table 1 The Effect of Cell Density and thus Differences in the Degree of Cell Contact on the Incorporation of Methionine and Ethionine into Protein

Cell concentration	^3H Methionine/ ^{14}C ethionine
6.9×10^4	107.9
1.5×10^5	95.0
3.5×10^5	101.3
1.4×10^6	86.9
2.1×10^6	102.7
3.1×10^7	94.7

The cells had been passaged thirty-six times.

radioactive counts in older cells probably reflects their reduced growth potential but it could give rise to differences in the counting efficiency when low levels of ^3H and ^{14}C are assessed in the same sample. Lack of discrimination in the protein synthesizing machinery would then be an artefact. When ethionine and methionine counts are plotted separately, however, the values do not decrease at the same time (Fig. 4); ethionine incorporation starts to rise at the point where methionine incorporation begins to fall. This supports the concept of mistranslation but also suggests that faults in informational macromolecules reach a measurable level before the degeneration of a culture. The efficiency of incorporation may also be affected by cell density and cell contact, both of which are much reduced in the older cultures. But even when cells were plated at a dilution where there was barely any cell contact a level of discrimination commensurate with the age of the cells was maintained (Table 1). A third possible explanation devolves upon the methylation of DNA^{27,28}. It has been shown that methyl and ethyl groups of DNA originate from ethionine and methionine²⁹ and changes in the methylation or ethylation of nucleic acids with age could well account for the results in Fig. 3. Such changes might well be connected with age-related modifications of chromatin^{30,31} rather than mistranslation. Samples were accordingly treated with DNAase and RNAase, but although there was a slight alteration in the total number of counts, the treatment did not materially alter the ratio in the protein fraction (Table 2). Changes in methyla-

**Fig. 2** Polyacrylamide gel electrophoresis to show the LDH isozyme distribution in cells of different ages^{24,25}. From left to right pass 19, pass 24, pass 35, pass 54 and a sample from MRC-5 cells transformed with SV40 virus.**Fig. 3** Changes with age in the uptake and incorporation into protein of an amino-acid and its analogue. Cultures of approximately 10^7 cells were pulsed for 24 h in medium containing $0.05 \mu\text{Ci ml}^{-1}$ methionine and $0.05 \mu\text{Ci ml}^{-1}$ ethionine.**Fig. 4** Total c.p.m. of ^{14}C ethionine (●), and ^3H methionine (▲) in the protein fraction of differently aged cells.

tion of nucleic acids therefore seem unlikely to be the cause of the reduced level of discrimination in ageing cells.

A further explanation which has not yet been investigated is that changes in the incorporation of ^3H methionine and ^{14}C ethionine arise as the result of alterations in the proportions of the various isoaccepting tRNA species³². A shift towards a species with greater affinity for ethionine might be equally responsible for the changes seen in Fig. 3 in which case the results would be rather more in line with theories which consider ageing to be a genetic programme related to differentiation^{33,34}.

But strong evidence that the changes in analogue incorporation are really associated with mistranslation comes from experiments with 5-fluorouracil^{34,35} for cells in medium containing this drug undergo changes which closely parallel those seen in ageing (Fig. 5).

Although none of these results conclusively support the error

Table 2 Ratio of ^3H and ^{14}C in the Protein Fraction after Treatment with DNAase and RNAase

Passage Number	Untreated sample			Treated sample		
	^3H Methionine	^{14}C Ethionine	Ratio	^3H Methionine	^{14}C Ethionine	Ratio
25	2,561	26	82.4	1,779	22	80.9
29	1,632	18	90.7	1,340	15	89.3
35	1,860	17	109.4	1,671	16	105.0
41	2,536	36	70.5	2,182	32	68.2
46	1,691	42	40.2	1,382	38	36.4
49	794	49	16.2	677	45	15.1
50	244	22	11.1	132	16	8.3
55	342	17	2.1	180	12	1.5

DNAase and RNAase treatment of the radioactively labelled fraction. Samples were treated for 2 h in 0.5 M Tris buffer pH 7.5 containing 0.33 mg ml^{-1} of DNAase, 0.51 mg ml^{-1} of RNAase and 100 mg of MgCl_2 .

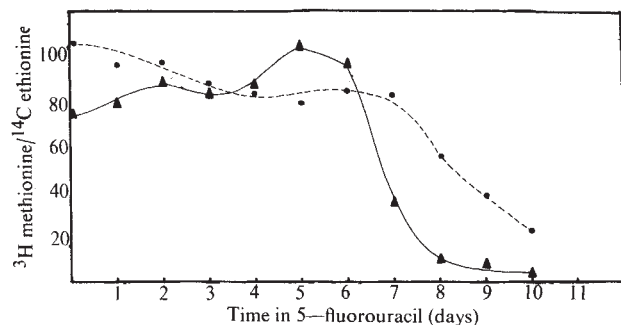


Fig. 5 The effect of medium containing 5-fluorouracil at 5 $\mu\text{g ml}^{-1}$ (▲), and 20 $\mu\text{g ml}^{-1}$ (●) on the incorporation of ^3H methionine and ^{14}C ethionine.

theory of ageing they strongly suggest that altered proteins accumulate in ageing cells. Whether this is the cause of death or whether it is merely associated with ageing is not yet known. Indeed, some results with 5-fluorouracil (to be published) indicate that MRC-5 cells are capable of "repairing" or preferentially removing their altered protein³⁶ even when almost half the protein of the cell contains an error. Errors may well be associated with ageing but we feel that at this stage it is premature to suggest that mistranslation is directly responsible for death.

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Lateral Heterogeneity in the Earth's Mantle

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The characteristics of seismic waves detected at the Large Aperture Seismic Array in Montana suggest that, beneath the Bonin Island Arc in particular, there are lateral inhomogeneities in the mantle

SEISMIC events can be located by a global network of seismic stations or "large array" techniques. The former gives more accurate locations, but the latter can contain valuable information about the Earth's mantle. We shall show in this article that a combination of the two techniques can also extract this information and that it is very likely that there are strong lateral velocity variations in the mantle.

The location of events by a global network of stations is a well established technique. With a standard set of travel times such as the Jeffreys-Bullen (J-B) tables, most events

recorded at ten or more stations at a variety of distances and azimuths can be located with an accuracy of about 30 km in the epicentre and 50 km in the depth. For the purpose of this article only, we shall assume that locations provided by the United States Coast and Geodetic Survey (CGS) in its bulletins of 1967 to 1969 are free of error.

Finding the positions of events with a large array such as the Large Aperture Seismic Array (LASA) in Montana is, however, done by fitting the best plane wave in the least squares sense, to the arrival times of the seismic P wave at the separate sub-arrays of the array. In this way the array determines the apparent azimuth and $dT/d\Delta$ of the incoming signal; a standard curve of $dT/d\Delta$ against Δ (distance)—derived from the J-B tables—then has to be consulted to give the distance of the event from the centre of the array and this, in combination with the azimuth, yields a location.

Many factors prevent the location by LASA being as accurate as the location by means of a global network. The time resolution on the seismogram of each sub-array is 0.05 s, and a realistic estimate of the accuracy of measurements would be 0.1 s. Clearly the most widely spaced elements of

the array are the most valuable for the purposes of location, and the signal, typically, takes 10 s to sweep across the array; this imposes limits on the precision of the location at about 100 km, but it will become clear that an array does not often give an accuracy approaching this value. We shall be particularly concerned with the causes of inaccuracy in array location.

Array Diagram

Fig. 1 is an "array diagram" for LASA. Each arrow on this diagram represents one seismic event. We have selected several hundred events, all well recorded at a common set of the outer twelve LASA sub-arrays. The tail of the arrow represents the $dT/d\Delta$ and azimuth of the least squares fit of a plane wave to the incoming signal at LASA with no corrections applied, and its head represents the $dT/d\Delta$ and azimuth that would be predicted from the CGS location and the J-B tables. Comparable diagrams have been presented by Weichert¹ and Vinnik and Nikolayev². The coastlines of the world, as seen from LASA with an array diagram on the assumption that the J-B tables are correct, and that the array is free of local bias, are shown in Fig. 2. Core phases are not used in this study.

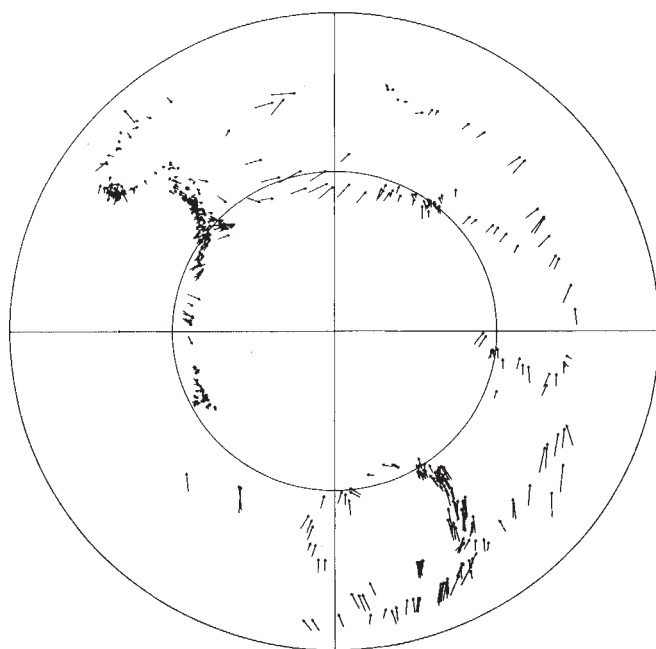


Fig. 1 The array diagram for LASA out to 10 s/degree. Core phases are not included and the data is thinned somewhat.

It is very clear from Fig. 1 that, if left to its own devices, LASA would seriously mislocate most seismic events and that there is no uniform correction that can be applied to the determinations of $dT/d\Delta$ and azimuth by LASA to improve their accuracy. From an operational standpoint this poses no problems as a compendium of these arrows can be stored and appropriate corrections applied before a bulletin is prepared. From the scientific standpoint, however, there are many important questions raised by Fig. 1. There are several possible causes for the deviations between apparent and real source locations in Fig. 1. First, if LASA lies above crust and upper mantle which are not laterally uniform, this structure will distort an incoming teleseismic plane wave. Part of this distortion can be considered as random corrugation of the wavefront, and there is ample evidence for this³. There may also, however, be a systematic term which can "tilt" the wavefront. Greenfield and Sheppard⁴ and Engdahl and

Felix⁵ demonstrated array-wide structure which would contribute to such a tilt and thus lead to errors of measurement of $dT/d\Delta$ and azimuth for the measured incoming wave. Such errors could be regarded as moving the tail of the arrows in the array diagram. Several models have been proposed for the crustal structure under LASA, and we shall take a very simple one as an example. If we used Engdahl and Felix's vertical travel time anomalies⁵ (their Fig. 8) as appropriate to describe LASA anomalies at all angles of approach (this is to be regarded as an example only) we would find the tails of all arrows in Fig. 1 displaced southward by about 0.4 s/degree. Other structures which are dependent on the angle of approach (such as a dipping Moho) would have slow smooth variations, for the array samples twelve such structures simultaneously and thus smooths out any rapid geological fluctuations beneath the array.

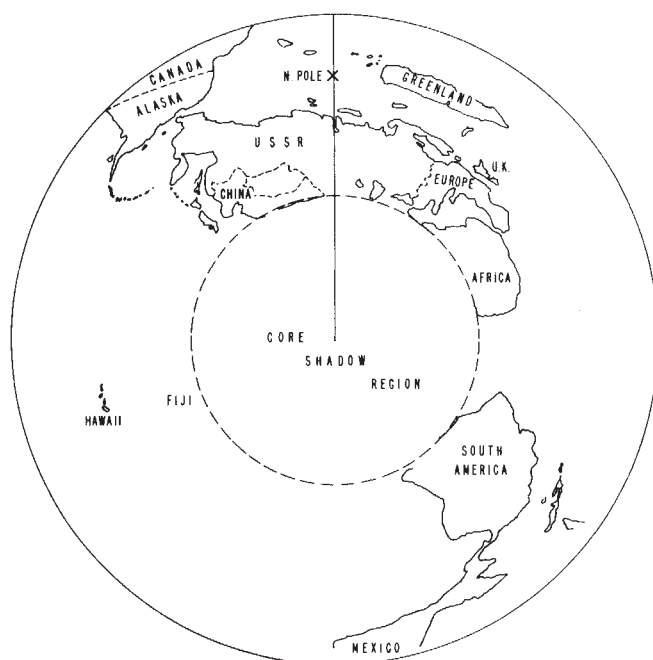


Fig. 2 The coastlines of the world as seen through the LASA array diagram and the J-B tables.

Second, values of $dT/d\Delta$ may be incorrect. We have used the J-B tables in placing the arrow head, but make no claim that they are right. If these tables are globally incorrect (that is, there is an error which is a function of Δ but not of azimuth) this will appear in the array diagram in the form of radial arrows, of length dependent only on radial distance. In this case one should regard the tail of the arrow as correct and the head, being the result of consulting incorrect tables, as in error.

Third, there may be lateral variations in velocity structure in the Earth. This comprises all lateral variations other than those so immediately beneath LASA that their effect is uniform for all incoming signals. There may, for example, be topography on the discontinuity at 630 km beneath LASA; there may be lateral variations in the deeper mantle; there may be lateral variations beneath the source. All these will also move the head of the arrow in the array diagram, because assumptions used in placing the arrow heads are no longer valid.

If there were a slope beneath LASA of 5° on the discontinuity at 630 km and a velocity contrast of 10%, and if this slope were not continuous over a region intersected by all bundles of rays from teleseismic events to LASA, then events from certain regions would have shifts in $dT/d\Delta$ and azimuth of up to 3% and 2° (ref. 6). This would be a slow,

smoothly varying phenomenon as LASA's 200 km window is convolved with such structure. Little is known of topography on the discontinuity at 630 km, but the assumptions probably represent the limit of any geophysically reasonable values in tectonically quiet regions.

In the vicinity of most earthquakes there are strong lateral variations in velocity. There are clear indications from many authors that beneath ocean ridges the P wave velocity is relatively low, perhaps by 10%, over a lateral and vertical extent of hundreds of km. There is equally good evidence that beneath trenches the P wave velocity in the consumed lithosphere is several per cent higher than that of its surroundings. Such contrasts of velocity near a source can clearly shift its apparent position, and it is a relatively simple matter to calculate the extent of this ray tracing. The problem is that of assigning reasonable structures, for little is known of the depth to which such anomalies extend. It is, however, our opinion, after studying correlations between mislocations and tectonic pattern and considering results from ray tracing calculations (B. R. Julian, personal communication) that azimuthal shifts of 2° and anomalies in $dT/d\Delta$ of 3% would generally represent the maximum that structure beneath the source could contribute. In particular, the effects of lateral variations decline with decreasing emergent angle to the vertical, and most of this study will deal with teleseismic waves that travel steeply towards the boundary between the core and the mantle. We shall, however, present evidence that seismic velocity structures near island arcs may, in one instance, have to be reconsidered.

The lateral variations in velocity near the bottoming point of seismic rays remain to be discussed. These can have a drastic effect on $dT/d\Delta$ and azimuth—much more so than on the travel time, which is relatively insensitive to small changes. In a spherically symmetric Earth $dT/d\Delta$ is a measure of the seismic velocity at the bottom of the ray and thus, for instance, if the values of $dT/d\Delta$ at which the core shadow was encountered were known, the velocity at the very base of the mantle could be inferred (with certain assumptions). If the lateral variation is very slow this will still be approximately possible in a more complex Earth, but as the scale of the variations diminishes the incoming signal has no longer traced a great circle path from source to receiver and the situation is much more complicated.

We thus have several possible reasons why the arrows in Fig. 1 should not be of zero length, and only a certain amount of intuition about how they might be separate. We shall show good cause, as we see it, to identify certain features in the array diagram with geophysical phenomena, but we can make no dogmatic statements. In the absence of corroborating evidence we can only indicate in each case what we believe is the most plausible explanation. Of course, refraction surveys beneath LASA and constraints on the (T, Δ) curve may help provide this corroboration. Nevertheless, we make no claim beyond plausibility. It is important to note that the data presentation may appear to be radically different from that of Chiburis⁷ and Engdahl and Felix⁵, who represented departures from the predicted plane wave at LASA in terms of time anomalies beneath each sub-array. The difference is more apparent than real. The data presented by time anomalies are much the same as those presented in the array diagram, and although the time anomaly technique contains information about corrugations in the wave front as well as about systematic tilting of the wavefront, it suffers from the lack of easy interpretation in terms of different sources of anomaly, and can lead to a belief that because the anomaly can be mapped on to a sub-array it must be caused by structure beneath that sub-array.

Closer Scrutiny

The first thing to be extracted from Fig. 1 is an array bias caused by structure immediately beneath LASA. It is not

immediately obvious that there is a systematic term in the confusion of Fig. 1, but a distribution of arrow directions and magnitudes in Fig. 3 makes it clear that the population has a general bias in the northerly direction of about 0.25 s/degree—that is, under the inevitable restrictions of non-uniform sampling we believe that the array structure can best be described as moving the tails of the arrows southerly by 0.25 s/degree. We apply a uniform correction of this magnitude to all arrows in subsequent figures.

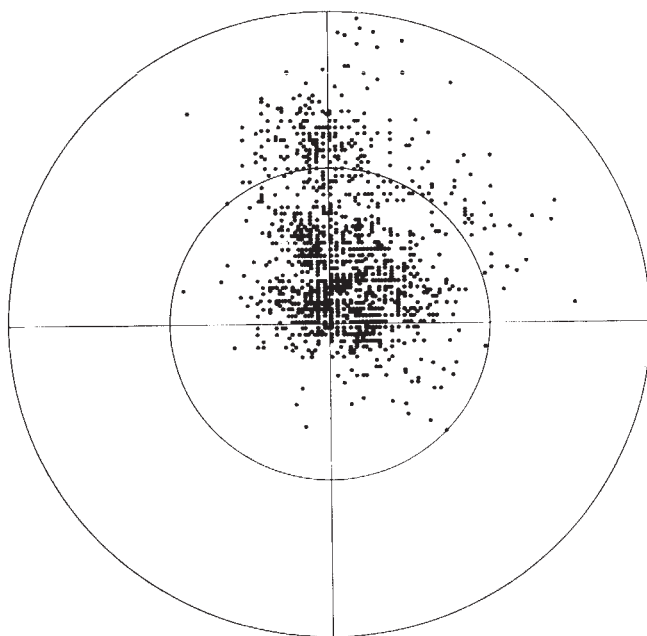


Fig. 3 The distribution of arrows in Fig. 1. The centre of this distribution is taken to be equivalent to a southerly shift of the arrow tails of 0.25 s/degree.

Fig. 4 shows the corrected array diagram in more detail (by sectors). In the discussion we shall refer to specific points in the diagrams by coordinates ($dT/d\Delta$, azimuth).

The first point to be made is that there are some regions where the arrows change very rapidly. It is clear that no structure beneath the array could yield such rapid changes unless the geological structure beneath individual sub-arrays for waves coming from these regions changed in unison for all sub-arrays in order to mimic an apparent change in arrow length and direction. This seems highly implausible, so we are led to believe that the rapid fluctuations must have their origin at some distance from the array. In addition to these rapid variations there are broader trends such as are seen in Fig. 4a and b, where there is a good sampling at $dT/d\Delta = 7.5$ s/degree. On the other hand, we see no compelling evidence to modify the curve of $dT/d\Delta$ from the J-B values. The apparently random distribution of radial and azimuthal arrows in the array diagram suggests to us that it is not profitable to attempt to reduce the scatter by the application of radial corrections only. It has to be concluded that lateral heterogeneity at all depths in the Earth's mantle must be considered as a reasonable hypothesis. Previous clear evidence of the non-uniqueness in curves of $dT/d\Delta$ against Δ has been given by Greenfield and Sheppard⁴ and Chinnery⁸.

It is unnecessary to go through all features in the array diagram in detail, but some are worth brief discussion. We may first note the complexity of the diagram for $dT/d\Delta$ less than 5.0 s/degree, that is for waves which, theoretically, have bottomed within 300 km of the boundary between core and the mantle, the region D' of Bullen. This has always been

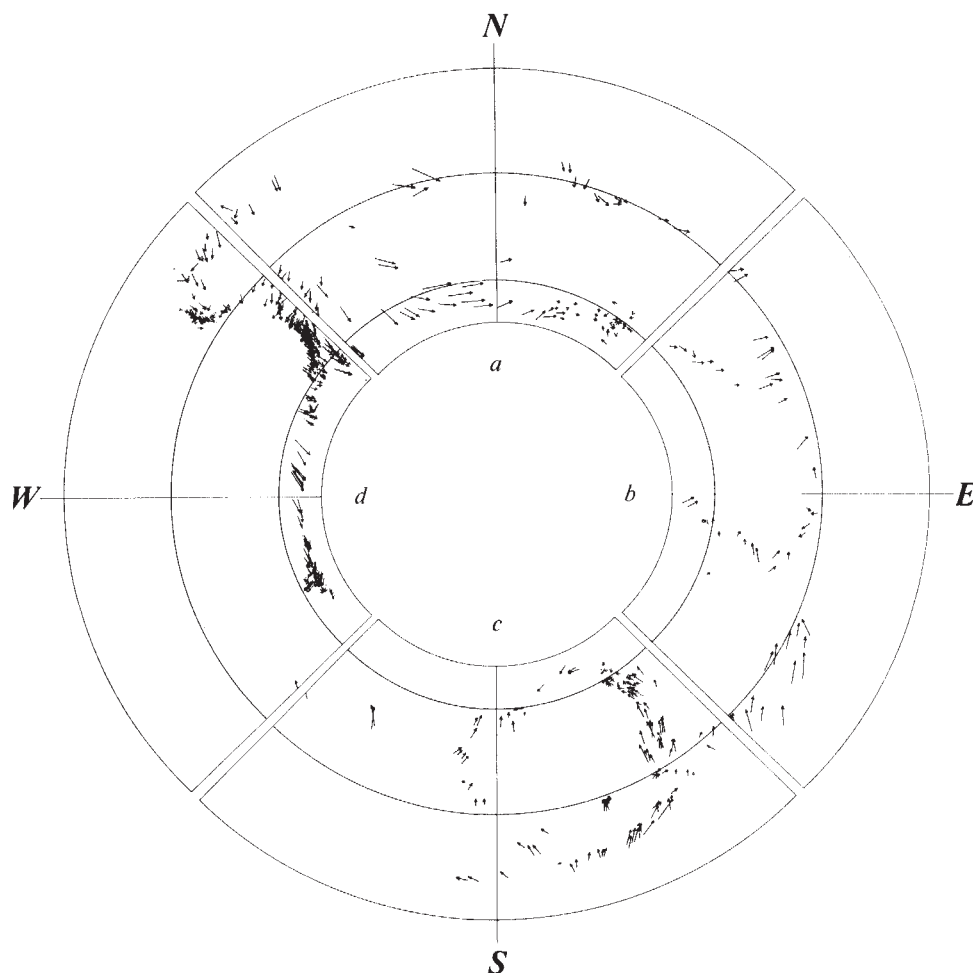
a problem region for seismology, and lateral heterogeneity in this zone has been suggested previously⁹.

We note that wherever there is sampling of this zone there are rapidly varying anomalies to be found. For instance in the azimuth range 240° to 315° arrows acquire a markedly different $dT/d\Delta$ and azimuthal term at about 265° . This effect is seen much more dramatically in Fig. 1. The addition of a constant makes visual differentiation more difficult. Fig. 5 is an enlarged portion of Fig. 1 in this region.

The degree of implausibility in associating such a rapid variation of anomaly with structure beneath the array is well illustrated by examining the data reported by Engdahl and Felix⁵ in their Fig. 4c and d. The rapid changes in sub-

$dT/d\Delta$ of 4.20 and 4.35 s/degree respectively. E. R. Kanasewich *et al.* (to be published) also report very low values of $dT/d\Delta$, down to 4.1 s/degree. Although this is all clear evidence that there is lateral heterogeneity near the boundary between the core and the mantle, it is not possible to convert limiting values of $dT/d\Delta$ directly to velocities just above the boundary as the presence of azimuthal anomalies implies that the Earth is not even locally purely radially stratified and that the Wiechert-Herglotz technique is thus inapplicable. At least two other regions give rapid variations for $dT/d\Delta$ less than 5.0. In both cases the azimuthal term seems to dominate the transition—at azimuths of 20° and 155° . The transitions in all cases seem to be effected within about 5° of azimuth

Fig. 4 a, b, c, d. Sections of Fig. 1 (bias removed) drawn on an expanded scale. The outer circle has a value of 10 s/degree, the inner circles are at 7.5, 5.0 and 4.0 s/degree.



array residuals correlate with the rapid changes in the arrows in the azimuthal range 240° to 300° , and the changes are apparent on all sub-arrays, which either requires that all sub-array structures change simultaneously or that the incoming wave vector changes. The latter is clearly a more reasonable alternative.

Now this transition also corresponds to a movement of the core shadow from about 98° for Kermadec events to at least 104° for New Guinea events. It seems that where there is good visibility beyond 100° (probably including diffracted P waves) $dT/d\Delta$ flattens off to a value of about 4.70 s/degree. On the other hand when the core shadow is at a shorter distance, $dT/d\Delta$ may drop to much lower values. Two good examples are the southern Mid-Atlantic Ridge and Persia. In both cases there is seismicity out to greater distances, so the shadow is well established at about 95° with values of

or 200 km laterally at the bottoming point in a laterally homogeneous Earth. If we seek an explanation in terms of deep mantle heterogeneity we can note that rays from the three azimuths of rapid contrast, 20° , 155° and 265° , bottoming at distances from LASA of about 48° , come in each case within 500 km laterally of passing through the projection of Iceland, the Galapagos Islands and Hawaii on the boundary between the core and the mantle. Morgan¹⁰ has proposed that these surface features may have a source deep in the mantle.

It is natural to try to associate these anomalies with wave propagation around the base of such a small scale heterogeneity. A rough calculation shows that if a ray arriving off the great circle azimuth by 5° is traced back to its bottoming point, at $\Delta = 45^\circ$ say, it will be separated laterally from the great circle trace by about 200 km. If we were to assume

that at exactly the bottoming point the ray were turned to continue on to the source, the total surface distance travelled by the ray in excess of the great circle path would be less than half a degree. If this ray were to come in before the great circle ray it would have to gain about 2 s relative to the latter. This could be accomplished if the latter passed through a low velocity zone at the bottoming point. Using the figure quoted above for 200 km, it turns out that if the low velocity zone had a horizontal radius of 200 km and was 6% lower in velocity, this could be achieved if the off-azimuth signal were diffracted round this zone. This calculation should only be regarded as an extremely rough indication of the quantities required to explain such propagation anomalies. Although three rapid transitions immediately appeal to us because of their proximity to proposed "hot spots" it must be added that there appears to be an equally striking anomaly involving high velocities (that is arrow tails point towards an azimuthal line) at an azimuth of 290°.

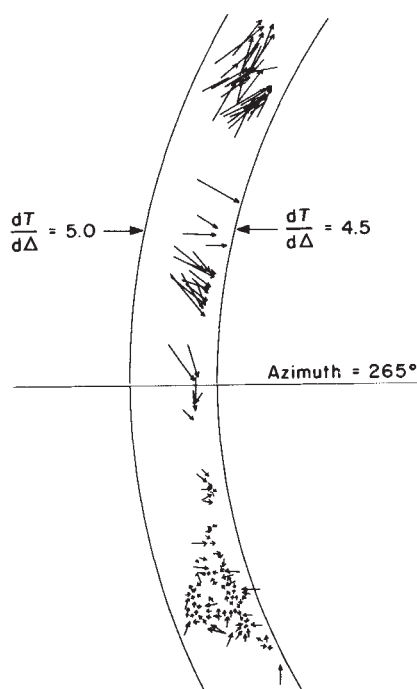


Fig. 5 A close-up of Fig. 1 near (4.5, 265°). (Data not thinned.)

Another obvious feature at $dT/d\Delta < 5.0$ s/degree is the broad anomaly from azimuth 340° to 10°, containing both azimuth and $dT/d\Delta$ terms. In order to sustain azimuthal anomalies over about 30° of azimuth it is necessary to postulate rather persistent lateral velocity gradients. These may be spread over the total region in which seismic waves occupy the deepest mantle and thus could be reduced to lateral velocity gradients of perhaps 1% a hundred kilometres or less. We should expect such gradients to be rendered visible by travel time seismology, although any lateral variation along the path from source to receiver will complicate the issue greatly through the introduction of anomalies in $dT/d\Delta$ which are clearly also visible in the data set in question. Travel-time evidence for lateral heterogeneity in this region is presented by M. K. Sen Gupta and B. R. Julian (to be published) and a total interpretation will require a joint analysis of data.

Broad anomalies and rapid changes are by no means restricted to waves that have bottomed in the deepest mantle, as is obvious from inspection of the array diagram. There are some particularly striking examples in the eastern hemisphere at

values of $dT/d\Delta$ of 7.5 s/degree and greater, all of which merit discussion in detail. We shall confine ourselves, however, to the singular anomaly near (9.0, 160°), where it appears that arrow tails are deflected inwards to an azimuthal line of 160°. From this we might infer higher velocities at depths of about 700 km along this line than on either side. Much of the anomalous pattern in the east at 7.5 s/degree likewise appears to be due to perturbations at depths of up to 1,500 km, and the bottoming points of these rays are quite close to the downward projection of the American continent/ocean boundary. We cannot rule out the possibility that this technique can identify deep-seated differences between continent and ocean which may be inaccessible to standard techniques involving travel times.

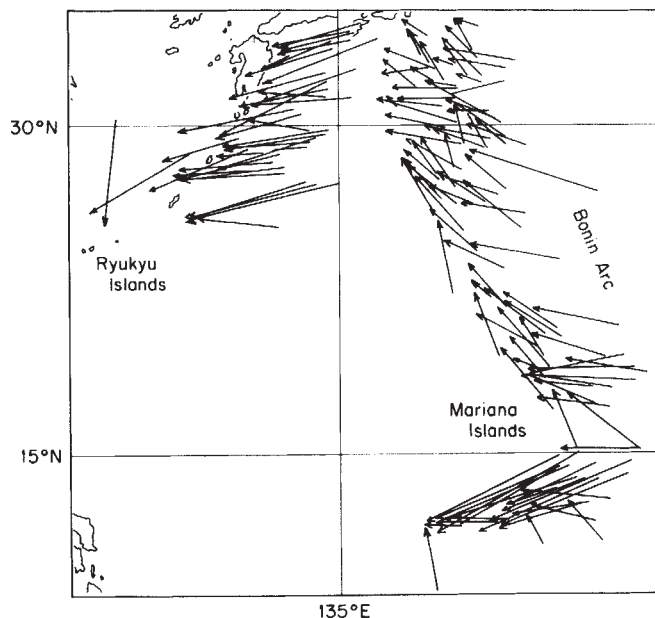


Fig. 6 A geographical projection of the Bonin Island Arc showing CGS and LASA (raw) locations (head and tail of arrow respectively).

One final feature should be remarked upon in this preliminary discussion. Near (5.0, 305°) in Fig. 4d there is a rapidly varying feature which has been a puzzle to us, as it was to Chinnery¹¹. It is not very clear in Fig. 4d, but a geographical projection shows up the problem in more detail (Fig. 6). At the triple junction off Honshu the mislocations for earthquakes of all depths in the Bonin arc retain their azimuthal term, but $dT/d\Delta$ changes sharply. At the other end of the Bonin Trench (Marianas Islands) the $dT/d\Delta$ term returns to values it has in the Ryukyu Trench south of Honshu. Clearly we could map this feature on the deep mantle, invoking rapid lateral transitions. The correlation with surface tectonic features is, however, striking—much more so than any other examples we have looked at. We have already commented that ray tracing techniques in a model of a descending slab do not produce large changes in wave vector and this is especially true for rays leaving the "rear" of the slab. We are led to consider an interface beneath the slab—perhaps the 630 km discontinuity which is now well established from seismic reflexions¹¹. The Bonin Arc is the only one we have examined in which the slab dips away from LASA (to the southwest), and the only explanation we propose is that perhaps the discontinuity at 630 km (with a contrast in the P wave velocity of 0.9 to 1.0) dips steeply behind the island arc. One pointer that such an explanation may be reasonable is that the anomaly in $dT/d\Delta$ does not obviously appear dependent on depth,

suggesting that the source of the anomaly is deeper than the deepest earthquake in this region (about 600 km). Calculations indicate that the dip of this anomaly source has to be of the order of 10° in the opposite direction to the dip of the slab. This would have serious geophysical implications connected with our understanding of seismic structure, mantle composition and phase change thermodynamics in the vicinity of island arcs. Further data need studying in the light of this hypothesis. In particular we must try to see if other island arcs show this phenomenon to any extent. In this instance, however, the hypothesis of structure near the source appears to us most plausible, and the postulate of a dipping discontinuity underneath the slab the simplest explanation. Fig. 7 shows a sketch of the proposal.

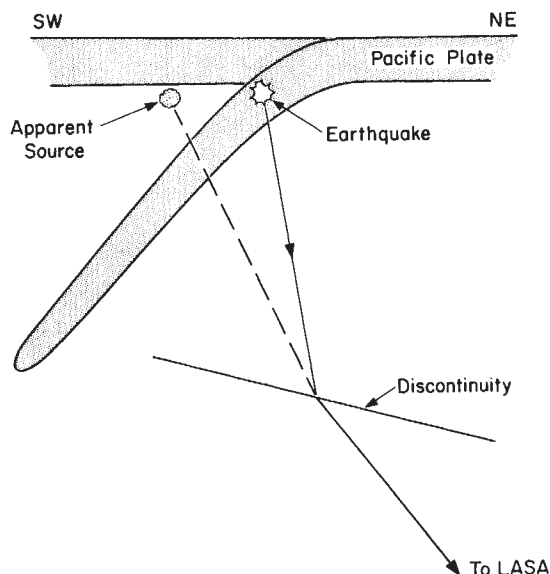


Fig. 7 Proposed structure beneath the Bonin Arc to explain the Bonin anomaly.

It may be protested that if one anomaly can be moved from the base of the mantle to a shallower discontinuity because of its striking correlation with surface features, perhaps other anomalies would admit of similar treatment even if the correlation with surface features is absent. This argument is unsatisfactory because a dipping discontinuity beneath the source cannot introduce the large azimuthal terms over the azimuth span required (up to 30°) without the layer breaking surface. Further, reducing the value of $dT/d\Delta$ to as low as 4.2 s/degree by dipping layers would also be impossible, for such a value of $dT/d\Delta$ would, in the absence of anomalous structure near the core, simply deflect the ray into the core. The fact that rays with $dT/d\Delta$ this low are seen is itself important.

Structure Revealed

Fig. 1 contains a large amount of information about structure beneath LASA and velocity heterogeneities both near to the sources, and deep in the mantle. Much of this information is difficult to extract in that different sources of anomalies overprint each other. Nevertheless we can make some deductions, which can be summarized as follows.

First, there is some evidence of structure beneath LASA, leading to smooth mislocation tendencies. We can see a mean bias which is in moderate agreement with previous studies.

Second, our study offers no improvement on the J-B curves of $dT/d\Delta$. Johnson¹³ and Corbishley¹⁴ have used arrays to propose new curves of $dT/d\Delta$, but in both cases the possibility

of azimuthal anomalies was not considered, and in both cases an attempt was made to remove the effect of structure beneath arrays. We believe that some valuable information may be lost if effort is concentrated on obtaining a curve of $dT/d\Delta$, and accordingly have not pursued this objective.

Third, there seems to be good evidence that seismic rays are turned sharply in the vicinity of some sources, and that this anomalous behaviour cannot be ascribed to the relatively well known velocity heterogeneities at present delineated beneath tectonic features. We propose as a seismically acceptable explanation that the underthrust slab in the region of the Bonin Islands influences a discontinuity such as the one at 630 km over a broad region, and that the discontinuity rises as the slab descends.

Fourth, there is strong evidence that lateral velocity variations occur in the mantle. Some of these, such as the anomalies associated with rays bottoming at 700 to 1,500 km, are as yet little understood, but may arise through contrasts between continent and ocean. Rays bottoming in the deepest mantle give a good coverage to LASA, and show a wide variety of rapid changes in azimuth and $dT/d\Delta$. $dT/d\Delta$ for events beyond 90° is not a unique quantity, and values between 4.20 and 4.70 s/degree are reported at 95° . The distance of the core shadow appears to be related to $dT/d\Delta$. If $dT/d\Delta$ is low, events beyond 95° are not seen but if $dT/d\Delta$ is high it is possible to see P waves of short period or P waves diffracted out to at least 104° . It thus seems necessary to propose that there are regions of strong velocity contrasts in the vicinity of the boundary between the core and the mantle.

Fifth, it seems that some of these contrasts have a small scale length at the boundary. Evidence from earthquakes in Tonga to the Solomon Islands suggests scale lengths as small as 200 km or less. On the other hand, evidence from Eurasian earthquakes reveals broader anomalies, of scale perhaps 1,000 km in the northeast quadrant.

Sixth, three of the regions in which anomalous deep mantle seems necessary to explain the seismic observation are in the vicinity of the downward extension of proposed "hot spots" of Morgan¹⁰.

Seventh, it is difficult to place quantitative estimates on the velocity perturbations, because we see no easy way to invert the relatively sparse data which include azimuthal as well as $dT/d\Delta$ terms. In this article crude and purely speculative calculations give some first ideas on possible velocity contrasts. Ray tracing seems the most fruitful approach and is being pursued at present. Travel times may also assist such an inversion.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Radio Continuum Emission at 21 cm near Stephan's Quintet

AN area about one degree square centred at $\alpha = 22^{\text{h}} 33.80^{\text{m}}$, $\delta = 33^{\circ} 41.0'$ (1950) near Stephan's Quintet of galaxies has been observed with the Westerbork Synthesis Radio Telescope at 21.2 cm. The measurements consist of 12 h registrations at 40 interferometer spacings from 36 to 1,440 m in intervals of 36 m. The observations were Fourier-transformed to produce a map of the radio brightness of the region with a synthesized beam 25 arc s wide at the half-power points. The results in the immediate neighbourhood of Stephan's Quintet are shown in Fig. 1. The radio contours are drawn over a photograph of the Quintet which has been taken from ref. 1. The radio features detected consist of an unresolved source (indicated by the white cross on the photograph) located very near the nucleus of the spiral galaxy NGC 7319, and an unusual, approximately semicircular arc of emission which appears to partially envelope this galaxy. Any radio emission from the nucleus of NGC 7320 is less than $2.5 \times 10^{-29} \text{ W m}^{-2} \text{ Hz}^{-1}$; a similar limit also applies to NGC 7317.

The nuclear source in NGC 7319 has a flux density of $28 \pm 5 \times 10^{-29} \text{ W m}^{-2} \text{ Hz}^{-1}$. The distance to this galaxy is presently a point of dispute; although the redshift of the galaxy² (6657 km s^{-1}) indicates a distance of 133 Mpc (for $H = 50 \text{ km s}^{-1} \text{ Mpc}^{-1}$), it has been suggested³ that the redshift is largely non-doppler and that the distance is instead similar to that of the nearby large spiral NGC 7331 (redshift 794 km s^{-1}). In any case, the nuclear radio emission does not seem to be unusually intense; in a study of a sample of interacting galaxies observed at Westerbork (including NGC 7319), B. F. Burke and colleagues (unpublished) have found that the ratio of optical to radio luminosity for these objects is similar to that of normal, non-interacting, spiral galaxies which are also radio sources. But even though the intensity of the nuclear radio emission from NGC 7319 may not be unusually great, it is interesting to note that it is just this galaxy which is partially enveloped by the arc of radio emission. The geometry of the unusual feature between NGC 7318 and NGC 7319 strongly indicates an association with NGC 7319 and argues against a simple chance superposition of an unrelated background radio source. The association of such a radio feature with a galaxy is unique as far as we know. The arc seems unresolved in a direction perpendicular to any tangent line; the angular thickness is certainly less than 10 arc s. There is, however, a faint extension to the west (shown by the 1.4 K contour) which appears to cover part of an arm-like optical feature in NGC 7318.

There are several possible interpretations of the radio arc. The emission may somehow be associated with the large difference in redshift between NGC 7320 (750 km s^{-1} according to Allen⁴, a value which is similar to that for NGC 7331) and the other four members of the Quintet ($\approx 6,700 \text{ km s}^{-1}$). Although this interpretation may be attractive at present (see, for example, ref. 3), it does not immediately suggest what further calculations or observations should be made in order to support it.

An interesting possibility is that NGC 7319 is "colliding" with NGC 7318. The radio emission could be caused by an enhancement of the normal spiral-arm synchrotron emission of NGC 7318 arising from compression of the interstellar relativistic particles and magnetic field. Because the motion is most likely to be highly supersonic (the observed radial velocities of NGC 7318 *a* and *b* and NGC 7319 are 6,689, 5,727, and 6,657 km s^{-1} respectively), this model may be amenable to a simple shock-front analysis in the manner outlined by Spitzer⁵. If we assume that the broad radio emission just west of the main ridge is the undisturbed spiral arm emission of NGC 7318, then a density compression factor of the order of 10 and a shock region thickness of a few hundred pc are suggested by this model. One possible problem is that we might expect to observe the presence of the dense region just behind the shock front in H α (for example) as the gas cools; however, filter photographs at the appropriate radial velocities do not show any measurable H α emission in this region (H. Arp, private communication). A further difficulty is simply the geometry of the arc; it clearly curves around to the north and south of NGC 7319 in directions where the relative velocity of the galaxies may be small.

The existence of the nuclear radio source in NGC 7319 suggests that the emission from the arc is caused by cosmic rays which are generated in the nucleus of NGC 7319, and

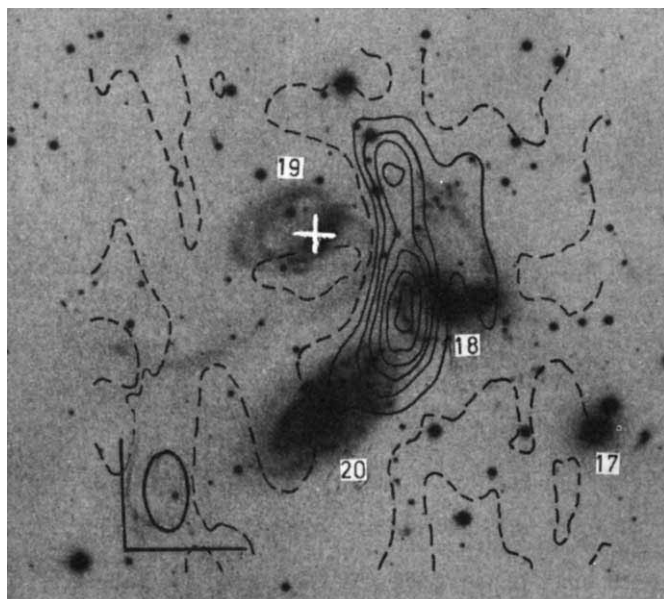


Fig. 1 A contour map of the 21 cm continuum radiation in the immediate neighbourhood of Stephan's Quintet, drawn over an optical photograph. The zero contour is shown dashed. The first contour and the contour interval are in units of 1.4 K beam-smoothed brightness temperature; this value corresponds to three times the r.m.s. noise fluctuations on the map. The white cross marks the radio position of an unresolved source; the telescope response has been subtracted at this position in order to remove the perturbing effects of the accompanying sidelobes on other features in the map. The ellipse at the lower left represents the half-power response of the telescope, and the L-shaped figure nearby gives the scale of one arc min.

which move outwards and radiate in a more dense interstellar medium associated with NGC 7318. A similar model was introduced by Ryle and Windram⁶ in order to explain the unusual radio "tail" features in the Perseus cluster of galaxies. The presence of bright HII regions in NGC 7318 and their absence in the spiral arms of NGC 7319 (see Fig. 1) indicates that the interstellar medium is indeed more dense in NGC 7318. A density ratio of at least 10 would be needed in order to account for the absence of radio emission within the arms of NGC 7319 itself provided that the cosmic rays are ejected isotropically from the nucleus. The chief difficulty with this model seems again to be the shape of the arc; unless the geometry is very special, we would expect the radio emission to be spread more uniformly over NGC 7318.

Perhaps the most attractive model is that the radio emission occurs in a galactic bow shock which arises from the gravitational accretion of gas by NGC 7319 as it moves through the group of galaxies. In this case the gas could either be that which was left over after the galaxies of the group condensed, or gas which has been expelled during the mutual gravitational tidal interaction of the pair of galaxies NGC 7318 *a-b*. The galactic bow shock model has been mentioned by Ruderman and Spiegel⁷ and computed in more detail by Hunt⁸. The radio emission could arise in a compression region, or through cosmic rays from the nucleus of NGC 7319 as described above. The problem of H α emission behind the shock would be alleviated because of the much lower gas density involved. The arc-shaped geometry occurs in a natural way from the structure of the bow shock itself.

Observations of the spectrum and linear polarization of the radio emission in this field are presently under way in order to test these models.

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Her X-1: A Precessing Binary Pulsar ?

RECENT observations of the two pulsating binary X-ray sources Her X-1 (ref. 1) and Cen X-3 (ref. 2) have led to a great deal of speculation about the X-ray emission mechanism involved. Because orbital parameters have been derived from their respective Doppler curves implying orbital radii R_0 of only 10^{11} to 10^{12} cm, it is natural to suppose that matter is accreted on one star from its companion. In the case of Cen X-3 with one binary member a massive star³ ($\sim 15 M_\odot$), the low-mass companion ($M \lesssim 0.1-1 M_\odot$) is presumably a collapsed object with high surface gravity (either a white dwarf, neutron star or black hole). So a large source of gravitational energy is tantal-

izingly available for radiation. If this were released during accretion of gas on the collapsed star (with mass M and radius R) at a rate dm/dt , the resulting steady state luminosity $L \approx (GM/R)(dm/dt)$. Even for a modest mass transfer rate, 20^{23} g yr⁻¹, the maximum probable X-ray luminosity $L_x \approx 10^{36}$ erg s⁻¹ can be accommodated (assuming $GM/Rc^2 \sim 10^{-1}$ as in a neutron star).

But no other property of the observed X-ray emission is easily explainable⁴. In particular, if the mass of the emitting object is low^{1,3}, then it is most likely to be a neutron star. Neither nonthermal bremsstrahlung of gas accreted onto a solid neutron star crust, nor thermal bremsstrahlung of a hot corona can easily account for the apparently "hard" spectrum¹ of the emitted X-rays in Her X-1. Further, even in a plausible binary accretion model⁵, there seems to be no natural explanation for the observed few-second X-ray pulsational period. Also, the high "Q" of the pulses, their short rise time, and the existence of an "interpulse" in Her X-1 are all difficult to understand on the accretion hypothesis. In the absence of a detailed accretion model, I propose a tentative phenomenological suggestion: that Her X-1 (and perhaps also Cen X-3) is a precessing binary pulsar.

The most remarkable property of Her X-1 is its pulsation. Its pulse period $T \approx 1.24$ s is roughly equal to the average period of the known radio-emitting pulsars. The existence of an interpulse, a common pulsar feature, is most suggestive. The only confirmed example of an X-ray pulsar, that associated with the radio pulsar NP 0532 in the Crab nebula, has an X-ray pulse and interpulse shape similar to that of Her X-1.

The X-ray Doppler curve clearly indicates the binary nature of Her X-1. In addition to its 1.7 day orbital period, a 36 day period has persisted throughout the year of its observations during which the X-ray source is undetectable altogether for about 27 days. If Her X-1 is a pulsar, emitting radiation in a pencil beam normal to its spin and equatorial axes, the 36 day period can be understood as a consequence of the free precession of the beam in and out of our line of sight. If θ_s is the angle between the principal body axis and the spin axis, and θ_b is the full angle of the pulsar beam, for a symmetrically placed observer $\theta_s \approx 2 \theta_b$ in order to accommodate the relative on and off durations of 9 and 27 day, respectively. But if Her X-1 appears to be "on" when it is at an extremum of its precessional motion, then no obvious connexion exists between θ_s and θ_b .

The free precession time T_p for an oblate spheroid with equatorial and polar moments of inertia I_e and I_p spinning in time T_s is given by

$$T_p = T_s / (\varepsilon \cos \theta_s) \quad (1)$$

where $\varepsilon \equiv (I_e - I_p)/I_e$. Because $\cos \theta_s \leq 1$, if the 1.24 s period is interpreted as the spin time T_s , and the 36 day period is identified with the free precession time, $\varepsilon \geq T_p/T_s \approx 10^{-6}$. With a neutron star of radius $R \approx 10$ km, the equatorial radius need only be 1 cm longer than the polar radius. Is this reasonable ?

A spinning non-viscous fluid will be oblate, with

$$\varepsilon \approx \frac{I\omega^2/2}{GM^2/R} \approx (T_s^2 G \rho/2)^{-1} \quad (2)$$

With $T_s \approx 1.24$ s and $\rho \approx 10^{14}$ g cm³, $\varepsilon \lesssim 2 \times 10^{-7}$, which is of the right order. Unfortunately, in the absence of external torques, the spin axis must be aligned with the total angular momentum vector and it will not precess. Still, if the neutron star cooled soon after forming, when presumably it was rotating faster than it is now, and if it solidified at that time, even if only forming a surface crust, it might well have retained an oblateness of the required order. At least one puzzle still remains, however: what subsequently uncoupled the total angular momentum vector of the pulsar L from the spin S ? Even in such a close binary system, tidal forces play only a negligible part in the dynamics of the collapsed star. For example, the tidal distortion of the neutron star due to its companion is of order $\varepsilon \approx (M/M_c)(R/R_0)^3 \leq 10^{-18}$ where M/M_c is the ratio of

masses of the neutron star and its companion at an orbital radius R_0 . The forced precession time, even for a highly oblate object, is orders of magnitude larger than the 36 day period ($T_1 \approx R_0^3/T_s \epsilon GM > 10^{10}$ s). Clearly, the precession of Her X-1 must result from the binary nature of the system, because no pulsar is observed to have such a long time-periodic behaviour⁶, and probably the precession results from non-gravitational forces in the system. Perhaps plasma, current or other spatially asymmetric electromagnetic forces resulting from the binary nature of the system can supply the torque necessary to initiate free precession.

It seems almost necessary that if Her X-1 is in fact a pulsar, it should be a pulsed radio source with a 1.24 s period. If pulsed radio frequency radiation is found, a dispersion measure could be determined, allowing both a distance estimate and an absolute flux determination to be made. One might expect that the companion star would be optically variable due to the heating of its surface by the pulsar and should continue to vary even when Her X-1 is "off" with respect to the 36 day period, because of the large solid angle subtended by the companion. Little can be added to these speculations without either a radio or optical identification of Her X-1, or some understanding of pulsars themselves. Even if no such identification is immediately forthcoming, there seems to be little more in favour of a binary accretion model than a binary pulsar model of the type proposed here to account for the properties of Her X-1.

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Note added in proof. After this letter was submitted for publication, B. J. Rickett and I searched for Her X-1 at radio frequencies with the UCSD radio telescope (3° diameter beam) at the centre of both the UHURU and MIT X-ray positions while Her X-1 was "on" at X-ray frequencies with respect to both its 1.7 and 36 day periods (June 1–2, 1972). A summed epoch analysis for ten-minute intervals revealed no pulsed component of the radiation at the 1.238 s period above the 2–3 σ level, leading to an upper limit on the flux at 74 MHz, if its pulse width is $\sim 1/5$ s, of about 1 flux unit. This observation does not disprove the pulsar nature of Her X-1, for if its binary companion has a corona (or there is mass exchange between the stars) a rather modest gas density $n \approx 10^8 \text{ cm}^{-3}$ will suffice to absorb all radio emission below 100 MHz. Such a corona will not absorb either X-ray or optical radiation but might itself emit a small amount of thermal bremsstrahlung at these wavelengths ($\lesssim 10^{32} \text{ erg s}^{-1}$).

At high frequencies, 2,695 and 8,085 MHz, radio observations made while Her X-1 was "off" at X-ray energies reveal the existence of four weak radio sources ($10^{-2} - 10^{-1}$ f.u.) lying within about a degree of the MIT X-ray position (R. Doxsey, G. T. Murthy, S. Rappaport, W. Zauman and J. Spencer, unpublished data). This is roughly the number expected for a random distribution of such weak sources over the sky. Unless the radio and X-ray "beams" have different sizes or point in different directions, the present pulsar model would suggest that these radio sources are unrelated to Her X-1.

A positive result at about the 5–8 σ level has been reported for optical variations (with a 1.238 s period) of a possible optical counterpart to Her X-1 called HZ Her, a variable star lying within the MIT X-ray error box, but having coordinates which are inconsistent with all of the suggested MIT radio source candidates (D. Q. Lam and J. M. Sorvari, *IAU Circular* 2422, July 17, 1972). Verification of this result, for example, by measuring changes in the optical pulse arrival time from the "pulsar", or by measuring spectroscopic variations in the companion (which should be observable even when Her X-1 is "off") would be further evidence for a binary pulsar model.

Finally, if Her X-1 is a pulsar, and the emission is non-uniform across the beam, the X-ray, radio and optical luminosities averaged over the 1.7 day "on" time should vary as the beam precesses over the 9 day "on" period. If the MIT X-ray position is correct, then one can interpret recent UCSD OSO-7

X-ray observations of Her X-1 as marginal evidence for this effect (M. Ulmer, private communication). In the present model, if the observer is symmetrically placed with respect to the precession axis, I expect the variation of the beam intensity to reverse its direction in time every 36 days.

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A Sediment Cloud Below the Mediterranean Outflow

DURING observations of the currents close to the sea floor below the Mediterranean outflow in the Gulf of Cadiz, we have discovered evidence of a cloud of sediment which seems to come from the Mediterranean water itself.

The warm salty water, which leaves the Mediterranean as an undercurrent through the Straits of Gibraltar, flows as a submerged jet in contact with the sea floor, around the northern side of the Gulf of Cadiz under the influence of Coriolis forces and bottom topography. It moves sediment and creates a variety of bed forms as it flows over the bottom (ref. 1; and Belderson and Kenyon, in preparation). To the east of 8° W in the Gulf, there are at least three submarine valleys running roughly NE–SW², and some of the lower part of the outflow is deflected down these valleys to depths of 1,000–1,400 m. The flow down one of these valleys at 36° 15.8' N, 7° 22.4' W sometimes exceeds 1 ms⁻¹, and close to the bottom it is highly turbulent. There is evidence of local scour from bottom photographs and from the presence of sediment caught in equipment above the bottom. At the foot of the valleys the flow resumes its course to the northwest and at 36° 12.5' N, 8° 02' W, the Mediterranean water is no longer in direct contact with the bottom, but flows as a free jet of variable structure (Fig. 1). Our measurements of suspended sediment were made at this location.

We measured the current at nine levels using Bergen current meters. As these meters are not suitable for measuring turbulence very close to the sea floor, an electro-magnetic flow meter was developed to measure the current almost to the bottom. To check its performance we mounted a camera on a tripod to photograph vanes which orientate to the flow and give a measure of the current. We used 16 mm film with an exposure time of 1 ms, starting the camera before lowering the tripod to the sea bottom, and running it until the film ran out after 2,400 exposures.

On March 17, 1971, the tripod was lowered to the sea floor taking photographs every 299 s. We found the image covered in fine marks, which were later identified as particles of suspended sediment. Fig. 2 is a typical frame from the film taken by the camera standing on the sea floor. The particles were first encountered at about 1,420 m during lowering, and continued to the bottom at 1,473 m, where the film showed their presence for over 81 h. The currents during this period were slow, 10 cm s⁻¹ or less at 1.2 m above the sea floor, and very variable in direction. Particles of sediment collected on the upper plate of the compass support and were later swept

away as the strength of the current varied. The particles were about 1 mm or less, with a flocculent appearance, rather like snow flakes. A temperature salinity-depth (TSD) profile made 2.5 h after starting the film (Fig. 1a) shows that the top of the sediment cloud lies just below the core of the high saline, high-temperature Mediterranean water.

On March 22 a second film (photographs every 14.8 s) again clearly showed the cloud sediment. The upper edge was first encountered between 1,210 m, where particles were very sparse, and 1,330 m, where they were plentiful (Fig. 2), far above the edge observed on March 17. The variation seems to be due to a change in the water characteristics, for a TSD profile one hour after the film was started (Fig. 2b) shows that the highly saline, high-temperature Mediterranean water has intruded at 1,150 m, and the sediment top is associated with the lower edge of this new water. The sediment may be confined to the lower part of the Mediterranean water either because it is this which is in contact with the bottom in the region where the sediment is carried into suspension, or

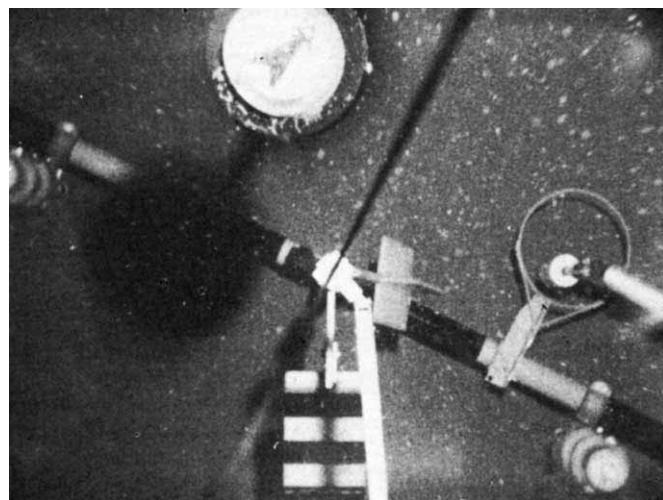


Fig. 2 Photograph showing the sediment in suspension above the bottom at 1654 March 22, 1971. The camera is 2.9 m above the bottom. The diameter of the black ring around the compass at the top is 8.9 cm. On it can be seen some sediment particles, which have collected in the lee of the raised transparent dome over the compass itself. The bar which diagonally traverses the photograph is 30 cm off the sea floor, and the stripes on it are 30 cm long.

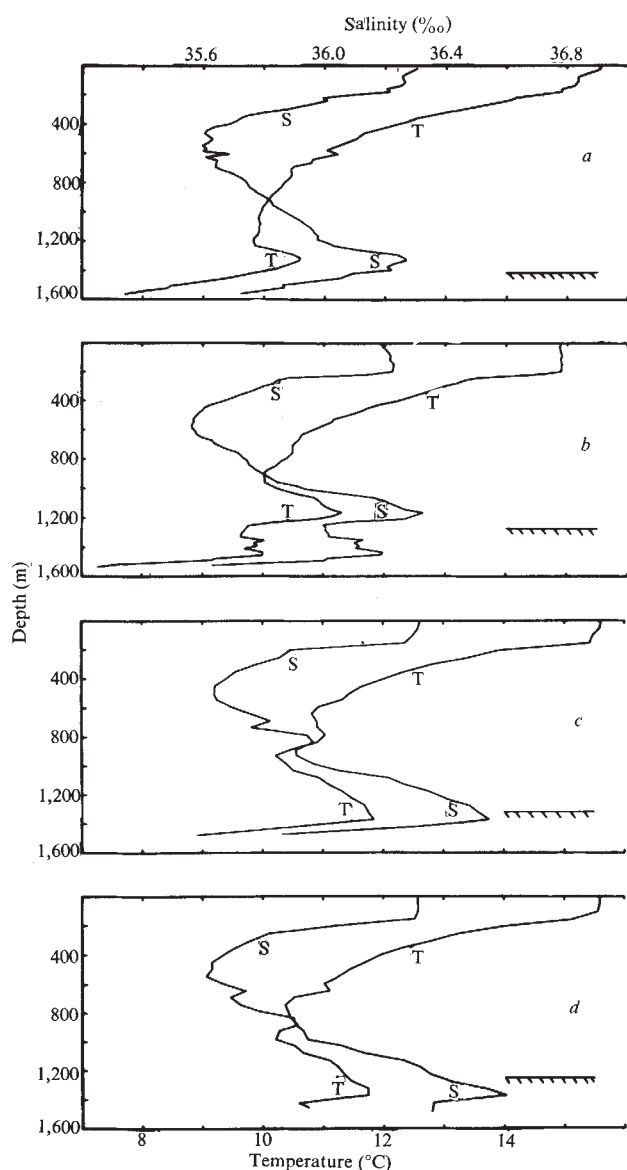


Fig. 1 Vertical profiles of temperature T and salinity S near $36^{\circ} 12.5' N$, $8^{\circ} 02' W$ at (a) 0234 March 18, 1971, (b) 1520 March 22, 1971, (c) 0902 March 1, 1970, and (d) 1926 March 3, 1970. The top of the sediment cloud measured at (a) 2304 March 17, 1971, (b) 1356 March 22, 1971, (c) 0750 March 1, 1970, and (d) 1820 March 3, 1970, is shown by the hatched horizontal line at the bottom right of each figure.

because, while flowing to the position of our observations, the sediment has fallen out of the upper region. The turbulent intensity measured by the electromagnetic flow meter 2.1 m above the sea floor was very low, and it seems unlikely that the sediment is locally eroded. The bottom seems to be a smooth area of sediment deposition and is composed of grey mud, with a sparse population of bottom living animals.

Two earlier films taken at the same location in March 1970 also showed a cloud of suspended sediment. Its upper edge was higher in the Mediterranean water than was found in 1971 (Fig. 2c and d). Whether the sediment cloud is a permanent feature in this area is not known.

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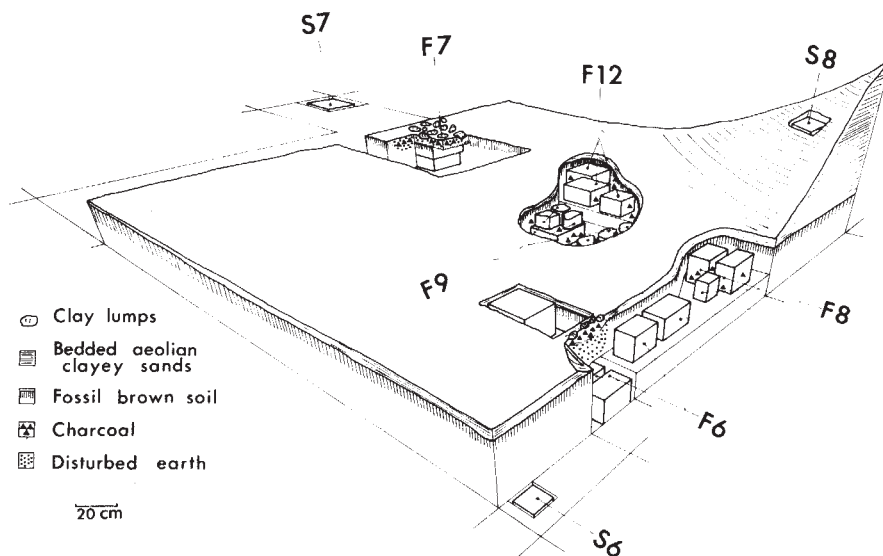
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Evidence of a Geomagnetic Excursion 30,000 yr BP

WE have carried out the first archaeomagnetic investigations of aboriginal fireplaces in Australia. Evidence of human occupation in Australia extends back to 25,000 to 32,000 BP, and is based on the remains of a human cremation found in the ancient lakeshore dune of Lake Mungo¹, a dried out lake in western New South Wales ($33.7^{\circ} S$, $143^{\circ} E$). We report here a study of some ancient fireplaces from an equivalent horizon around Lake Mungo; these fireplaces have preserved a record of a recent excursion of the Earth's magnetic field.

The geomagnetic field has reversed frequently in the past, and details of polarity changes for 4.5×10^6 yr are well established². The present Brunhes normal epoch began 0.69×10^6 yr ago, but there have been reports of a number of short period reversed events during that time. Careful studies of deep sea

Fig. 1 Schematic diagram of the sampling area at Lake Mungo showing the locations of blocks of baked soil (F), lumps of baked clay, unbaked dune sediments (S) and charcoal samples.



sediment cores show the existence of the Blake event between 0.108 and 0.114×10^6 yr BP (ref. 3). There is also the suggestion of another short reversed event between 0.33 and 0.35×10^6 yr BP (ref. 4). A young event has been reported from studies of lava flows in the Chaine des Puys, France, and called the Laschamp event⁵, but the exact date is uncertain. A study of Late Pleistocene sediments in California indicates that no polarity reversal lasting more than 1,700 yr occurred between 13,300 and 30,400 yr BP (ref. 6). This suggests that the Laschamp event occurred either before or after this period, and K/Ar ages on the lava flows indicate an age of less than 20,000 yr (ref. 5). Mörner, Lanser and Hospers⁷ report a reversal in varved marine clay and silt from Gothenburg, Sweden (the Gothenburg event). That event ended roughly 12,400 yr ago, and although its duration is not yet known, it is unlikely that it began much more than 13,300 yr ago. The Gothenburg event may be related to the Laschamp event⁷.

Ancient aboriginal fireplaces are common in central eastern Australia. Many of these are ovens—cooking stones or lumps of baked clay arranged on top of a thin band of ash and charcoal in a shallow depression. Other fireplaces (hearths) lack cooking stones, and consist only of ash and charcoal overlying baked earth. This investigation arose from a palaeointensity study of an unoriented baked clay lump from an oven (F6, Fig. 1) in the horizon around Lake Mungo estimated to be about ~30,000 yr old. During the course of thermal demagnetization experiments, we noticed that the sample contained a viscous component of magnetization in a direction opposed to the primary magnetization. Instead of decaying successively with increasing temperature, the intensity of magnetization first increased in value before decreasing at higher temperatures. There are two ways in which this could have arisen, either the sample was not in the original position in which it was fired, but had rolled over, or the primary magnetization was opposed to the present field direction, in which the viscous magnetization would be directed. We have found that consistent directions are almost always obtained from several stones collected from the same oven, so there seemed every possibility that a very recent reversal of the geomagnetic field could have been recorded. We therefore carried out a detailed investigation of several fireplaces in the immediate neighbourhood (Fig. 1).

The fireplaces are in an horizon buried beneath undisturbed aeolian clay sands. Samples of friable baked soil were obtained by carving around blocks and setting them in polyurethane foam. The samples were oriented by both sun-compass and magnetic compass before removal. Lumps of baked clay from an oven (F7, Table 1) were also oriented, and all the charcoal from each fireplace was collected for ^{14}C

dating. We also took oriented samples from the (unbaked) dune sediment stratigraphically above and below the horizon containing the fireplaces (see Fig. 1). Cylindrical specimens ($2.8 \text{ cm} \times 2.8 \text{ cm}$) were cored and sliced from each sample, and hardened in a solution of waterglass where necessary. All specimens were thermally cleaned by heating to 100°C and cooling in zero magnetic field⁸. This is sufficient to obtain the precise direction of magnetization, but still allows the retention of specimens for future Thellier double heating palaeointensity studies⁹.

The cleaned directions of magnetization of all the oriented samples are listed in Table 1, and the ^{14}C dates obtained for each fireplace are summarized in Table 2. Three of the fireplaces (F6, F7 and F8) and the unbaked dune sediment show

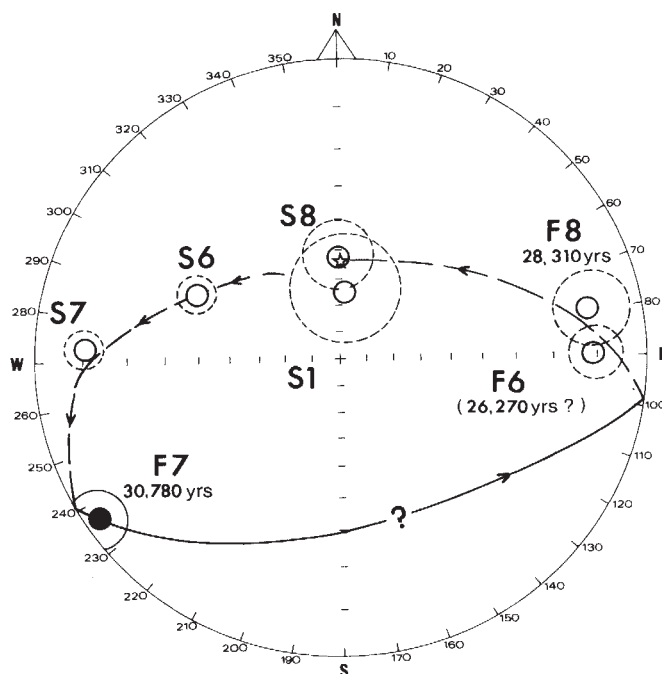


Fig. 2 Stereographic projection of site mean cleaned directions of magnetization from the three fireplaces (F) and unbaked sediments (S) which show good internal consistency. Open circles indicate negative inclination and solid circles positive inclinations, around which are drawn the circles of 95% confidence. The star indicates the direction of the axial dipole field ($0, -53.2$) at the locality. The time sequence of changes in direction of the geomagnetic field is indicated by the arrows, starting at S1 and ending at S8.

Table 1 Directions of Magnetization from Lake Mungo after Thermal Cleaning at 100° C

Fireplace or sediment	Sample	<i>n</i>	Sample mean <i>D</i> (°)	<i>I</i> (°)	<i>D_m</i> (°)	Site mean <i>I_m</i> (°)	<i>k</i>	α_{95} (°)	Position of virtual north pole
F6	J	3	88.9	-7.3	89.6 (<i>N</i> =3, <i>R</i> =2, 9959)	-10.6	490	5.6	237° E, 3° N
	K	9	91.6	-13.8					
	L	5	88.4	-10.6					
F7	A	2	232.0	+8.3	236.4 (<i>N</i> =4, <i>R</i> =3, 9904)	+3.2	313	5.2	34° E, 28° S
	C	4	237.7	+1.6					
	E	12	238.3	+2.4					
	F	7	237.4	+0.3					
F8	A	2	84.4	-11.0	79.1 (<i>N</i> =4, <i>R</i> =3, 9744)	-11.1	117	8.5	232° E, 12° N
	B	9	70.6	-17.5					
	C	11	78.8	-8.2					
	D	7	82.4	-7.4					
F9	A	5	132.8	+1.3	—	—	—	—	—
	B	8	119.1	+3.8					
	C	1	106.8	-1.6					
	D	3	100.4	+2.4					
	E	3	96.4	-1.7					
	F	2	102.8	-2.7					
	G	6	109.9	+10.0					
F12	A	12	257.4	-6.1	—	—	—	—	—
	B	9	224.6	+0.2					
	C	4	256.8	+0.3					
S1	A	2	—	—	4.9	-64.5	167	19.5	308° E, 77° N
S6	A	5	—	—	295.2	-35.6	249	4.9	49° E, 31° N
S7	A	4	—	—	272.8	-10.4	567	3.9	50° E, 5° N
S8	A	3	—	—	359.7	-52.3	121	11.2	126° E, 89° N

n is the number of specimens measured, *N* the number of samples and *R* the length of the resultant of *N* unit vectors. *D*, *I* are the declination (east of true north) and inclination (positive downwards) for each sample and *D_m*, *I_m* the corresponding values for site means. Sample means are too scattered at fireplaces F9 and F12 for a meaningful site mean to be calculated. *k* is the Fisher¹³ precision parameter and α_{95} the radius of the circle of 95% confidence about the mean direction.

good internal consistency in directions of magnetization. These are illustrated in Fig. 2, which shows wide departures from the axial dipole field (*D*=0, *I*=-53.1°) by as much as 120°. The direction listed for F6 (Table 1) may be older than the ¹⁴C age, because the baked earth samples were taken from a zone 15 cm below the apparent floor of the oven (the intervening zone being mixed ash and sand). Samples from the other two fireplaces (F9 and F12) give less consistent results (Fig. 3), but support the pattern of a large departure from the axial dipole field direction.

Table 2 ¹⁴C Ages for Mungo Fireplaces

Fireplace	¹⁴ C sample No.	Number of samples	¹⁴ C age (yr BP)
F6	ANU-667	1	26,270 ± 470
F7	ANU-680	3*	30,780 ± 520
F8	ANU-681	2	28,310 ± 410
F9	ANU-682	3	27,530 ± 340
F12	ANU-683	2	28,000 ± 410

* One sample counted for only 1,000 min.

Column 3 gives the number of benzene liquid scintillation samples prepared from charcoal and counted for 3,000 min each. Mean ages were calculated using a half life of 5,568 yr (ref. 14), and are listed together with their standard errors. Details of preparation and counting techniques are given by Polach, Lovering and Bowler¹⁵.

Assuming that the dune sediments record the direction of the geomagnetic field at the time of deposition (either depositional or chemical remanent magnetization) and that the baked soil

and clay lumps of the fireplaces have a thermoremanent magnetization dating from their last heating, then their stratigraphic positions and ¹⁴C ages make it possible to suggest a sequence of directions with time (Fig. 2). Sediment from well below the fireplaces (S1) gave directions close to the axial dipole field, but the directions observed in the unbaked dune sediments (S6 and S7) from horizons immediately underlying the fireplaces suggest the start of a polarity transition from normal to reversed. A deviation of 120° away from the axial dipole field direction occurred at 30,780 yr BP (as recorded by fireplace F7). At 28,310 yr BP (2,470 ± 660 yr later) the direction of the field was ~80° away from the dipole field and roughly opposed to that at 30,780 yr BP. Some time after that the field returned to normal (as recorded by S8).

Bucha¹⁰ examined some Pleistocene sediments from Czechoslovakia, and his data show evidence for a geomagnetic excursion at ~32,000 yr BP and lasting less than ~4,000 yr. This agrees well with our estimate of the age and duration of the excursion as recorded by the fireplaces at Lake Mungo. Moreover, Bucha's studies indicate the absence of a reversed event between ~30,000 and ~13,000 yr BP, and support the conclusion of Denham and Cox⁶ that no reversed event or excursion (lasting longer than 1,700 yr) occurred during this period.

The geomagnetic excursion we report is apparently not related to the Laschamp event which, according to K/Ar dating, ended less than 20,000 yr ago⁵. There is a lack of supporting evidence for any reversed event during the past 27,000 yr in Aegean deep sea cores¹¹ and during the past 13,000 yr in Lake Windermere sediments¹². This suggests that the Laschamp and proposed Gothenburg events (possibly both expressions of the same "event") were extremely short—and possibly excursions of the geomagnetic field.

We suggest that unusually large excursions of the geomagnetic field ($\sim 120^\circ$ or more away from the axial dipole) are unlikely to be due to secular variation (even at a time of low field intensity), but are more likely to be a manifestation of some fundamental property of the geomagnetic dynamo. Whatever their cause, there seem to have been at least two unusually large geomagnetic excursions during the past 35,000 yr.

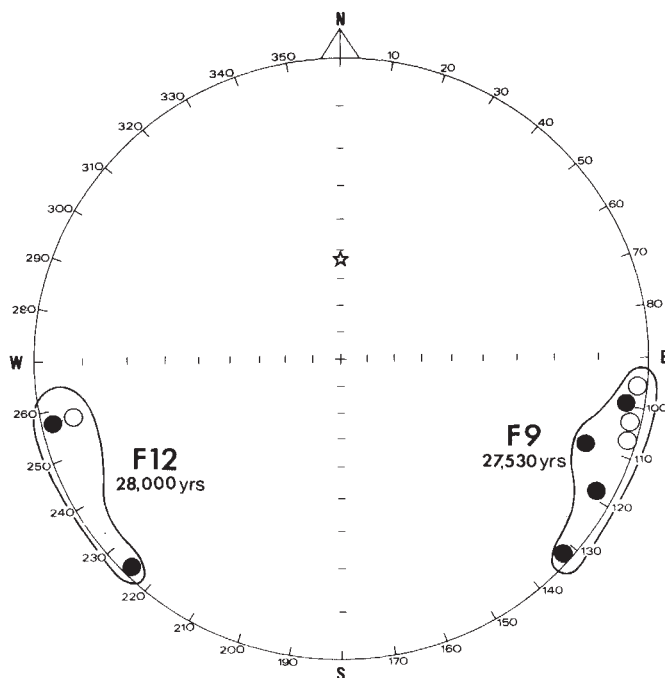


Fig. 3 Stereographic projection of sample mean cleaned directions of magnetization from the two fireplaces which do not show good internal consistency. These results do, however, provide general confirmation of the large departures from the axial dipole field direction shown in Fig. 2. Symbols are the same as in Fig. 2.

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Subduction Zones: Not Relevant to Present-day Problems of Waste Disposal

SUBDUCTION zones are considered to be sites of disposal for vast areas of the Earth's surface¹, while new surface is generated simultaneously at rise crests². Bostrom and Sherif³ suggest that the world's industrial and domestic waste be dumped into subduction zones at deep sea trenches to allow nature to complete the recycling process at geologically rapid rates of 5 to 10 cm/yr. They also point out that trenches are often sites of rapid rates of deposition and suggest that the dumped wastes would, speaking geologically, soon be buried. Francis⁴ suggests that canisters of toxic chemical and radioactive wastes could be dumped onto trench sediments and be expected to sink at rates of 20 m/yr, assuming that the mass of turbidites in the trench fill often spontaneously liquefies on shaking by earthquakes. The assumption is based on the supposed lack of evidence for deformed sediment in trenches. I will argue that the suggestion of Bostrom and Sherif³ is not useful for the next few dozen generations of human populations and will point out observational evidence to show that Francis's⁴ assumption is incorrectly founded.

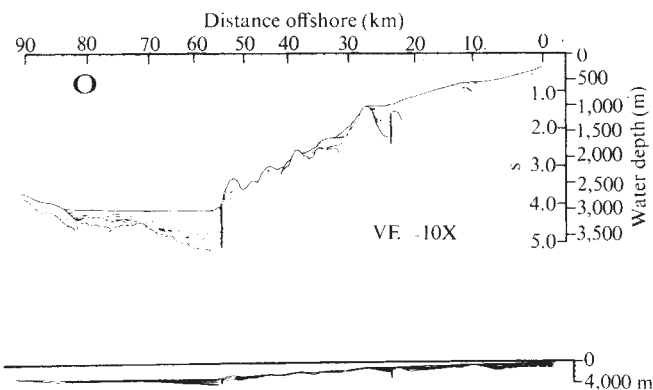


Fig. 1 Line drawing interpretation of reflexion profile across the continental slope off southern Oregon. West is left. Lower profile is true scale.

Refuse dumped into a trench will probably be spread over a minimum width of 1 km from the trench wall, provided navigation is excellent, though in practice this distance might be greater. Assuming all material reaching the trench wall is subducted—a questionable assumption as discussed below—then it will take 10^4 yr to subduct 1 km of sea floor at 10 cm/yr. Measured and estimated high rates of deposition of Pleistocene and Holocene sediment in trenches approach 1 m/1,000 yr^{5,6} and at these rates several thousand years would probably be required to cover a thin layer of refuse. Thus, for one to ten thousand years the trench as a waste disposal site would be subject to the same problems as any other deep sea site.

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Reflexion profiles across the Lesser Antilles Trench⁷, the Nankai Trough off south-eastern Japan⁸, the eastern Aleutian Trench⁹, the eastern Bay of Bengal¹⁰, and the continental margin off the north-western US^{11,12} show that deformation of trench turbidites begins along a rather sharp interface. These areas are all seismically active¹³.

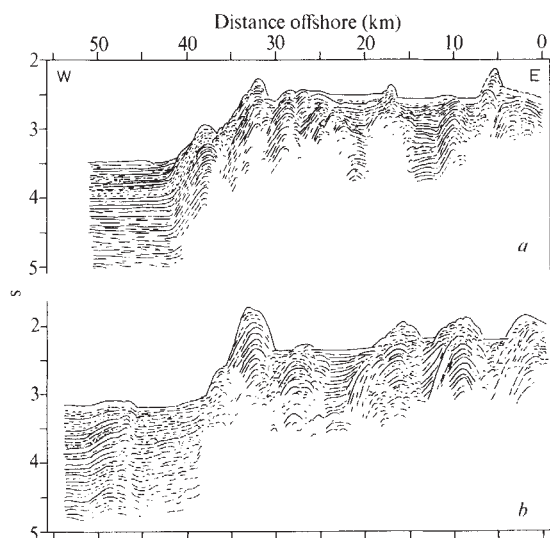


Fig. 2 Line drawings of profiles across the outer continental slope off (a) southern Washington and (b) central Washington. $\times 10$ vertical exaggeration.

Fig. 1 illustrates the abrupt transition from undeformed to deformed strata at location 54 km. Fig. 2a shows that turbidites and interbedded pelagics are turned up along the base of the slope to maintain an apparent dip of 10° . In Fig. 2b a reverse fault of small displacement cuts the basin strata at location 48 km. This perturbation may mark the beginning of a new ridge in the process of tectonic outbuilding of the continental margin, and such topographic relief is unlikely to be preserved in a medium that in Francis's model frequently attains a bulk viscosity of 10 poise⁴. Thus debris is unlikely to sink in this area. Similar arguments can be made for the surface relief associated with turbidite channels in the Cascadia Basin^{5,14}, the Bengal Fan¹⁰, the Peru-Chile Trench¹⁶, and the Aleutian Trench¹⁵. Turbidites are continuous across the entire Bay of Bengal and are deformed west of the Andaman Islands¹⁰. It is unlikely that the whole Bay undergoes spontaneous liquefaction. Thus the observational arguments for periodic liquefaction of sediments do not apply. The profiles from slow subduction zones show tectonic accretion of turbidites against the continental slope, suggesting that garbage dumped into the trench may pile up against the slope before being subducted.

In the case of the Peru-Chile Trench, the type example cited by Francis⁴, Seyfert¹⁶ suggested a sharp transition between undeformed and highly deformed strata to explain the structural geometry. Perhaps only drilling will solve the enigma first posed by Scholl and others¹⁷ for observations in the Peru-Chile Trench, but observations from the north-eastern Pacific, the Lesser Antilles, the Nankai Trough, the eastern Aleutian Trench and the Bay of Bengal suggest that subduction zones may be poor places in which to dispose of mankind's garbage and toxic wastes.

I thank D. S. McCulloch, J. E. Schoellhamer, D. W. Scholl, M. E. Silver and T. J. Wilcox for discussions and reviews.

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Reply

SILVER's contribution is in part a reply to an earlier one of mine and seems to be inspired by the fear that toxic wastes might immediately be dumped onto trench sediments. I do not, of course, suggest any such rash approach. Nevertheless, if liquefaction of sediments by earthquakes occurs, it would undoubtedly lend itself to waste disposal and this alone justifies some effort in looking for it.

My suggestion that sediments might undergo periodic liquefaction in the vicinity of earthquakes does not rest entirely on the interpretation of seismic reflexion profiles. Our knowledge of the spectra of ground motion close to earthquakes and of the physical properties of unconsolidated deep-sea sediment makes the concept event more compelling. I did, however, emphasize that the phenomenon would probably be confined to a particular sediment type—turbidite—especially if rapidly deposited or disturbed by frequent earthquakes. Thus the questions is not whether liquefaction occurs but how widespread it might be. For the phenomenon to be important for waste disposal the whole sedimentary column must liquefy. Liquefaction confined to particular strata within the column might be more likely.

In Silver's Fig. 1 I would agree that the fault at 54 km is a boundary between deformed and undeformed strata. But this does not imply that unconsolidated sediment is only affected by seismic vibration on one side of the fault. Weakening of sediment will occur on both sides of the fault, but only on the slope side can gravity bring about downslope movement and deformation.

Finally, if liquefaction occurs on the time scale I envisaged of the order of $s\ y^{-1}$ it cannot be expected to mask large scale normal faulting. Here it would merely smear the surface expression of a fault.

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BIOLOGICAL SCIENCES

Crystal Structure of a Novel Curariform Agent

THE belief that optimal competitive neuromuscular blockade occurs with compounds having two quaternary centres 12.5 Å apart^{1,2} has been progressively weakened by the demonstration³ that (+)-tubocurarine is a monoquaternary compound, by the X-ray studies⁴ on a potent curarizing steroidal derivative* in which the interjacency is 11.1 Å and by the even closer centres (9.7 Å) in the similarly active diallyl *bis*-nortoxiferine. Further evidence that an interquaternary distance of 12.5 Å is not critical in competitive neuromuscular blocking drugs is provided by the novel 1,1'-azobis-arylimidazo[1,2-*a*]pyridinium bromides, in which this interjacency is close to 7.5 Å.

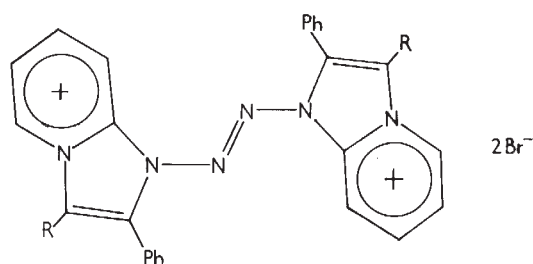


Fig. 1 (1), R = H; (2), R = Me.

Fig. 2 View of the molecule along the *a* axis. Carbon atoms are unlabelled, small circles are hydrogen atoms.



These results indicate that either cellular components bearing acetylcholine receptors are capable of more varied conformational changes than hitherto supposed, or effective competitive neuromuscular blockade can be achieved by single point interaction with receptors. The new class⁶ of bisquaternary compounds—illustrated in Fig. 1 by compounds (1) and (2)—possess potent, selective, short-acting curariform activity in several species⁷. We are concerned here with the precise stereochemistry of the crystalline dibromide of one such compound (1); its close analogue (2) is under clinical examination. The salt $[C_{26}H_{20}N_6]^{2+}2Br^-$, *M* = 576.3, crystallizes from

water as yellow rectangular prisms in the monoclinic space group $P2_1/c$ (C_{2h}^5 , No. 14) with $a = 9.15(1)$, $b = 12.05(1)$, $c = 12.61(1)$ Å, $\beta = 121.90(3)^\circ$, $U = 1180$ Å³, $D_c = 1.62$, for $Z = 2$, $D_m = 1.63$. Crystals smaller than the optimum size (0.4 mm) were used and 1,698 reflexions were obtained from Weissenberg photographs using Cu-K α radiation.

A three-dimensional Patterson function readily afforded the position of the bromide ion, and an electron density synthesis phased on this atom revealed the other atoms.

A full-matrix least-squares refinement with isotropic temperature factors gave *R* as 0.16 which decreased to the present value 0.112 on giving all atoms anisotropic temperature parameters. The absence of suitable sites for solvent molecules was verified using a Fourier programme but a difference map did not indicate chemically reasonable positions for hydrogen atoms.

The projection (Fig. 2) shows that the molecule consists of two identical halves related by a centre of symmetry located on the N(10)–N(10') bond. The imidazo [1,2-*a*] pyridinium ring system is nearly planar and both halves of the molecule connected by the 2-tetrazene linkage deviate only slightly from planarity. The least-squares best plane of the heterocyclic ring system and that of the phenyl ring are inclined at an angle of 47.5° to each other. The bromine anions lie above and below the nearly planar imidazopyridinium system (Fig. 2) at 3.60 Å from N(1) and 3.7 Å from N(4). The intramolecular distance N(4)–N(4') between the atoms most likely to carry the positive charge is 7.48 Å. The distance between the centres of the

pyridinium rings (the presumed centres of the dispersed charge) is marginally greater at 7.65 Å.

The limited charge delocalization (Fig. 1) indicated by our examination of the crystal structure contrasts with the point charge found in other curariform agents.

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Growth-promoting Effects of Silicon in Rats

To study new trace element deficiency diseases and to identify hitherto unrecognized essential trace elements, an all-plastic trace element controlled isolator system has been created^{1,2}. It prevents access of the animals to metal, glass, rubber, dust, or caretaking personnel. Each isolator accommodates thirty-two rats in individual plastic cages. Complete, chemically defined diets are used which are based on amino-acids in lieu of protein. The known essential elements are supplied in sufficient and balanced quantities. All ingredients are monitored for trace element contaminants. By means of this approach we have been able to demonstrate recently that tin^{3,4}, vanadium⁵, and fluorine⁶ (K. S. and D. B. M., unpublished results) are essential for growth.

The trace element sterile isolator prevents access to silicon, the element second in natural abundance in the biosphere. Under normal conditions animals and man are exposed to relatively large but variable intakes of this element through food, water, and dust. We have detected highly significant effects of silicon on growth of rats maintained on low-silicon diets in the isolator system (Table 1). The structure of the skull and pigmentation of the incisors were also affected.

Table 1 Growth Effect of Dietary Silicon on Rats Maintained in a Trace Element Controlled Environment on Low Silicon Diets

	Number of animals	Average daily weight gain	% increase over controls	P value
Basal diet A				
control	15*	1.51 ± 0.11 †		
50 mg% Si	11	2.02 ± 0.08	33.8	<0.005
Basal diet B				
control	12*	1.19 ± 0.06		
50 mg%	11	1.49 ± 0.06	25.2	<0.005

* Summary of two experiments.

† Average ± standard error of the mean.

Technical details have been described elsewhere^{1,2,5}. Inbred, litter-mate, male weanling Fisher 344 rats were used. To prevent intake of trace elements from the mothers' feed, chow pellets were removed from the nursing mother on the seventeenth day post-partum. The young were weaned and shipped to the laboratory on the nineteenth day. They were kept in plastic cages on the basal amino-acid diet for one day before initiation of the experiment in the isolator. Tests were carried out either on four groups of eight, or five groups of six animals per trace element-sterile isolator. As an additional control to each group in the isolator, six rats were maintained in metal cages under conventional conditions on each of the test diets. Food and water were given *ad lib*. At weekly intervals, food cups inside the isolator were replaced by clean cups containing freshly made diet. The animals were weighed and their appearance evaluated at three to four day intervals. Growth rates and standard errors of the means were computed by covariance

analysis of the weight gains. At 26 days the rats were rated for deficiency symptoms and autopsied.

Two amino-acid diets, A and B, were applied. The silicon level in these was below 5 p.p.m., determined by a modification of King's method^{6,7}. The composition of diet A was identical to that used previously⁵ except for additions of 1 p.p.m. of tin as stannic sulphate, $\text{Sn}(\text{SO}_4)_2 \cdot 2\text{H}_2\text{O}$, and 0.1 p.p.m. of vanadium as sodium orthovanadate, Na_3VO_4 . The composition of diet B was basically the same as that of diet A but it contained 15% of purified Wesson oil in place of stripped lard, 2 p.p.m. of tin as stannic sulphate or trimethyl tin chloride, $(\text{CH}_3)_3\text{SnCl}$, 0.5 p.p.m. vanadium as sodium orthovanadate, Na_3VO_4 , and 2.5 p.p.m. fluorine as potassium fluoride, $\text{KF} \cdot 2\text{H}_2\text{O}$. The salt mixture was changed to adjust sodium and potassium levels and the acid-base balance, in g per 100 g of diet, 2.1500 of CaHPO_4 , 0.2375 of K_2HPO_4 , 0.2420 of MgSO_4 , 0.01315 of $\text{MnSO}_4 \cdot \text{H}_2\text{O}$, 0.00685 of ZnCO_3 , and 0.000538 of KI. Sodium and potassium were provided by 1.11% of monosodium glutamate. H_2O and 2.3% of monopotassium glutamate. H_2O , supplied in the amino-acid mixture. Supplements of silicon as sodium metasilicate $\text{Na}_2\text{SiO}_3 \cdot 9\text{H}_2\text{O}$, were added as freshly prepared aqueous solution.

Addition of 50 mg of silicon per 100 g of diet produced highly significant increases in the growth rate of 33.8% in diet A, and 25.2% in diet B. Lower levels of silicon gave lesser, statistically insignificant responses. It seems that relative to other trace elements the amounts of silicon needed are quite large. It is possible that other silicon compounds occurring in natural materials would be more effective.

Growth responses similar to those presented here were found in 1956 during work on the trace element controlled environment system. It was suspected that the effects were caused by trace element contaminants in the silicate supplement. Having identified tin, vanadium, and fluorine as requirements for the rat we are now confident that the effects are produced by silicon itself, and that silicon is essential.

Animals on the amino-acid diet inside the controlled isolator showed an impairment of incisor pigmentation, indicating a disturbance of enamel development or pigment deposition in teeth. Significant improvements of tooth pigmentation were produced by silicon supplements. Pigmentation was measured on a standardized colour scale on day 26 of the experiments⁸. Fifty mg% of silicon caused a 21% improvement in pigmentations while 10 mg% gave a 16% difference in the colour score. Silicon, however, was only partially effective in preventing the impairment of pigment deposition. Significant effects on the incisor pigmentation are also produced by physiological levels of tin, vanadium, and fluorine.

Several other signs of deficiency in these rats, such as loss of hair, seborrhoea, and loss of tonicity, were not affected by the presence of silicon in the diets. Grenz-ray pictures of the skulls of the silicon-deficient rats demonstrate changes in structure. These skulls are shorter and the bone structure surrounding the eye socket appears distorted.

The distribution of silicon in blood and tissues was measured in controls. Silicon levels in blood are around 1 p.p.m.⁷. In parenchymal organs such as liver, kidney, lung, muscle, and brain, 2–10 p.p.m. are reported. Results reported before the use of plastic equipment are very much higher. The silicon content of bone and possibly also ligaments is much higher, with values around 100 p.p.m. The silicon content of bone may, however, depend on its age.

Carlisle has shown by electron probe microanalysis that up to 0.5% of silicon can be found in the rather narrow region of active calcification, that is, in the area of growth in young bone⁹. This suggests a physiological role for silicon in the initiation of the mineralization process. He found silicon produced growth responses in chicks fed amino-acid diets in plastic isolators¹⁰. These effects were similar to those we observed in rats. The silicon-deficient chicks showed retarded skeletal development and skull deformations.

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Blood Pressure Decreased by Halobutylpicolinic Acid, an Inhibitor of Dopamine β -Hydroxylase

THE isolation and purification of dopamine β -hydroxylase^{1,2} and the elucidation of the role in the biosynthesis of nor-adrenaline has been described already. The susceptibility of the enzyme to chemical inhibition has stimulated widespread interest in the use of enzyme inhibition *in vivo* as a pharmacological tool or an antihypertensive drug. Disulphiram, through its reduced metabolite diethylthiocarbamate (DDC), is an effective inhibitor of dopamine β -hydroxylase^{3,4}. No inhibitors of dopamine β -hydroxylase had been used successfully in the treatment of hypertension in man until 5-butylpicolinic acid was tried.

Fusaric acid (5-butylpicolinic acid), which is about ten times more potent as an inhibitor of the enzyme than DDC, is a clinically useful antihypertensive drug in nontoxic doses⁵⁻⁷. In a study of several 5-alkylpicolinic acids, reduction of blood pressure was more marked when a stronger inhibitor of dopamine β -hydroxylase was used⁸. Unfortunately, all the compounds tested in this study were less inhibitory than 5-butylpicolinic acid. We report here, however, studies with 5-(3',4'-dihalobutyl)picolinic acid or 5-(3'-halobutyl)picolinic acid, which inhibits dopamine β -hydroxylase *in vitro*, alters the tissue catecholamine content of the hypertensive rat and normotensive rabbit, and reduces their blood pressure consistent with *in vivo* inhibition of the enzyme more markedly than 5-butylpicolinic acid.

Fusaric acid and 5-(3',4'-dihalobutyl)picolinic acid or 5-(3'-halobutyl)picolinic acid were donated by Banyu Co., Japan. The hypertensive rats were adult males derived from the colony of spontaneously hypertensive rats (SHR) produced by selective inbreeding of a strain of Wistar rats^{9,10}. Systolic blood pressure in the tail was measured indirectly in unanaesthetized rats plethysmographically¹¹. Rats were prewarmed for 10 min and kept in a thermostatted box at 38° C during pressure measurement. The average of four readings was recorded. Blood pressure of rabbits was recorded from the carotid artery of animals anaesthetized with urethane, using a statham transducer and direct-writing recorder.

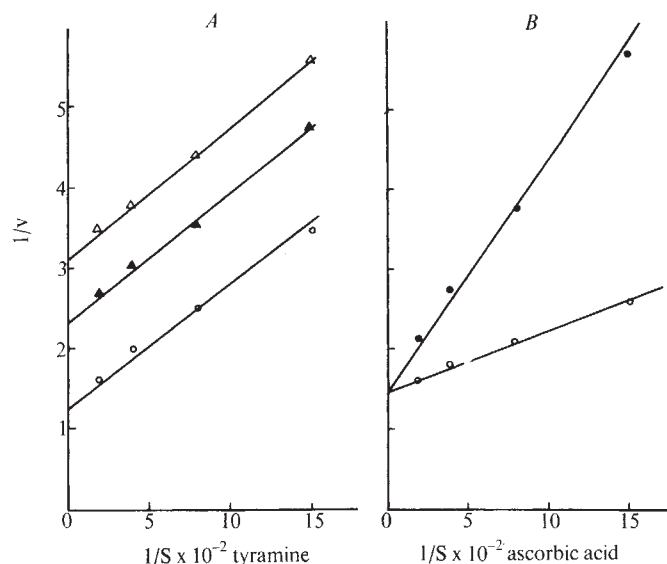


Fig. 1 A, Lineweaver-Burke plot of tyramine concentration (M) against rate of the β -hydroxylation of tyramine with and without Br_2FA . The assay was carried out as described in the text. The velocities are expressed as $A_{330\text{ nm}}$. $\circ$, Enzyme alone; $\blacktriangle$, enzyme with 5×10^{-9} M Br_2FA ; $\triangle$, enzyme with 1×10^{-8} M Br_2FA . B, Lineweaver-Burke plot of ascorbic acid concentration (M) against rate of the β -hydroxylation of tyramine with and without Br_2FA . The velocities are expressed as $A_{330\text{ nm}}$. $\circ$, Enzyme alone; $\bullet$, enzyme with 3×10^{-9} M Br_2FA .

Drugs were suspended in 0.25% aqueous methylcellulose and administered orally to the hypertensive rats. For the rabbits, drugs were dissolved in physiological saline, neutralized by NaOH and injected intravenously. Animals were killed by decapitation.

Each tissue was removed quickly and frozen at -20°C until assayed. Tissue catecholamines in the hypertensive rats or normotensive rabbits were determined by fluorometry^{12,13}. Dopamine β -hydroxylase was isolated from bovine adrenals and purified by ammonium sulphate precipitation, charcoal treatment, calcium phosphate gel treatment and DEAE-cellulose column chromatography, as described by Friedman and Kaufman². The spectrophotometric assay using tyramine as the substrate was used routinely to determine enzyme activity¹⁴. Enzyme activity was also checked using dopamine as the substrate¹⁵. Inhibition of the enzyme by the drugs for these two substrates (tyramine and dopamine) was compared and found to be almost the same as reported previously⁶. Concentrations of inhibitor were decreased within the range of 1×10^{-6} to 1×10^{-8} M until the inhibition decreased below 50%. The inhibitor concentration producing 45–55% inhibition (I_{50}) was determined graphically.

Inhibition of dopamine β -hydroxylase by 5-butylpicolinic acid and halogen substituted 5-butylpicolinic acids was investigated. All the halogen substituted acids were more inhibitory than the unsubstituted compound. The effects of 5-(3'-halobutyl)picolinic acid and 5-(3',4'-dihalobutyl)picolinic acid on enzyme inhibition *in vitro* were inhibitory to the same extent. The I_{50} of the halogen substituted 5-butylpicolinic acid was from 1×10^{-8} M to 2×10^{-8} M while that of 5-butylpicolinic acid was 8×10^{-8} M. Mechanism of dopamine β -hydroxylase inhibition by 5-(3',4'-dibromobutyl)picolinic acid (dibromo-fusaric acid; Br_2FA) was investigated. Inhibition of the enzyme by Br_2FA was competitive with ascorbic acid and uncompetitive with the substrate (Fig. 1). The substitution of halogen for hydrogen at 3',4' position of 5-butylpicolinic acid, however, seems to increase the capacity of the compound to penetrate the active site of the enzyme.

The oral administration of Br_2FA caused 50% fatalities in mice at 250 mg/kg eliciting marked hypotensive effects,

Table 1 Effect of Fusaric Acid and Dibromo-fusaric Acid on Level of Catecholamine and Blood Pressure in Spontaneously Hypertensive Rat

Time after drug injection (h)			Control	Fusaric acid (% of change)		Br ₂ FA (% of change)	
3	Blood pressure		197 ± 3.1	118 ± 10	(-40)†	133 ± 7.4	(-32)†
	Heart	NA	0.56 ± 0.02	0.40 ± 0.05	(-29)†	0.42 ± 0.07	(-25)†
	Adrenal gland	A	670 ± 47	595 ± 62	(-11)*	580 ± 50	(-13)*
6		DA	7.4 ± 0.5	8.0 ± 0.9	(+8)	10.8 ± 0.7	(+46)†
	Blood pressure		193 ± 5.4	132 ± 9.6	(-32)†	127 ± 6.9	(-34)†
	Heart	NA	0.50 ± 0.04	0.40 ± 0.07	(-20)†	0.34 ± 0.03	(-32)†
	Adrenal gland	A	715 ± 30	635 ± 97	(-12)*	580 ± 93	(-19)†
		DA	8.1 ± 1.3	8.7 ± 2.3	(+7)	17.5 ± 7.1	(+116)†

Each value represents the mean (µg/g or mm Hg) ± s.e. of eight determinations. The inhibitors (50 mg/kg) were injected orally to SHR and systolic blood pressures were described. 0.25% aqueous methylcellulose was administered orally to the control animals. A, Adrenaline; NA, noradrenaline; DA, dopamine.

* Significant difference ($P < 0.01$) from control.

† Significant difference ($P < 0.005$) from control.

while the administration of fusaric acid caused 50% fatalities in mice at 110 mg/kg. A comparison of the antihypertensive effect of Br₂FA and FA on SHR following a single administration at a dose of 50 mg/kg respectively (Br₂FA, 0.15 mmol/kg; or fusaric acid, 0.28 mmol/kg) is shown in Table 1.

The decrease in blood pressure of normotensive rabbits after the administration of 50 mg/kg fusaric acid and 47 or 94 mg/kg Br₂FA was observed in the anaesthetized rabbit, using direct blood pressure recording from the carotid artery (Fig. 2). These results also indicate that the effect of Br₂FA on the blood pressure of normotensive rabbit is more long-lasting than that of fusaric acid.

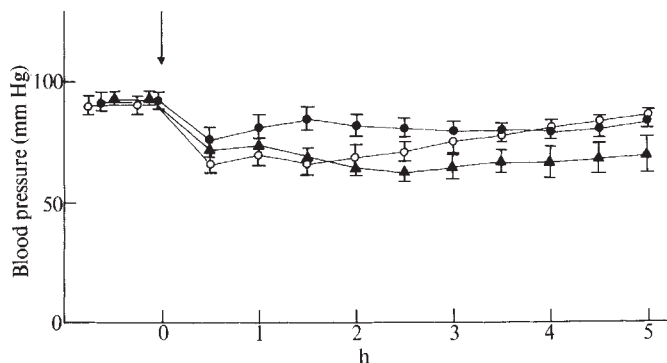


Fig. 2 Effects of fusaric acid and dibromo-fusaric acid on the blood pressure of the normotensive rabbit. Values shown are ranges (brackets) of results of 6 determinations. The inhibitors (50 mg/kg fusaric acid, 47 mg/kg Br₂FA, 94 mg/kg Br₂FA) were injected intravenously to rabbits at 0 time. Systolic blood pressures were recorded.

In view of the depletion of tissue noradrenaline and the reduction of blood pressure by fusaric acid, it was not surprising that Br₂FA, the analogue of fusaric acid, which is a more potent inhibitor, was also effective as a depletor of noradrenaline and an antihypertensive drug since it undoubtedly inhibited the enzyme *in vivo*, as did fusaric acid. This also suggests that the effect of Br₂FA on blood pressure is mediated through a sympathetic blocking mechanism. Although the functional role of copper in the activity of purified dopamine β-hydroxylase^{1,2} renders this metalloenzyme vulnerable to chelating agents, the inhibition of dopamine β-hydroxylase by fusaric acid and Br₂FA is probably not due to chelation of the copper in the enzyme. The inhibition produced by the binding of DDC and Cu²⁺ ion was overcome by the addition of exogenous Cu²⁺ ion², while the inhibition produced by fusaric acid or Br₂FA was not counteracted by the addition of Cu²⁺ ion. These data indicate that 5-butylicolic acid and its derivatives are another group of inhibitors of dopamine β-hydroxylase *in vitro* and *in vivo*. All the halobutylicolic acids inhibited the enzyme *in vitro* more markedly than 5-butylicolic acid.

This marked inhibition by halogen-substituted compounds can probably be best explained by the hypothesis that halogen at 3' or 4' position in 5-butylicolic acid would be the moiety in these inhibitors which is a prerequisite for effective chelation.

A significant reduction of blood pressure in normotensive man coincident with the peak of the concentration of plasma fusaric acid and consistent with significant increase of urinary dopamine and homovanillic acid excretion was demonstrable in man¹⁷. Terasawa and Kameyama⁷ reported that fusaric acid was an effective antihypertensive agent. The stronger inhibitory activity of Br₂FA than of fusaric acid may explain the fact that Br₂FA is more long lasting and potent in lowering blood pressure. Ogata *et al.*¹⁸ reported that fusaric acid has a property to counter the effect of alcohol and also suggested that this drug may be useful for the treatment of chronic alcoholism.

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Opposite Effects of L-Dopa and 5-HTP on Spinal Sympathetic Reflexes

HISTOCHEMICAL mapping of monoamine-containing neurones in the spinal cord has established the course and terminations of abundant descending neurones that contain either noradrenaline (NA) or 5-hydroxytryptamine (5-HT). Although these neurones terminate throughout the spinal grey matter, the intermediolateral columns receive the most dense terminations of both NA and 5-HT neurones that appear to make intimate synaptic contact with sympathetic preganglionic neurones^{1,2}. The possibility that sympathetic outflow is influenced by these monoaminergic neurones is strengthened by the locations of their cell bodies in areas of the caudal medulla oblongata that are involved in central vasomotor control³.

The monoamine precursors, L-dopa and 5-HTP, produce marked changes in transmission through spinal somatic pathways that are attributed to formation and release of the putative neurotransmitters, NA and 5-HT, respectively⁴⁻⁸. The reported presynaptic leakage of excess amine from monoaminergic terminals after precursor loading^{2,9} should have effects which mimic those of normal release. Therefore, we have used this approach to test possible influences of monoamines on sympathetic out-flow. Changes in excitability of sympathetic preganglionic neurones produced by monoamine precursors were determined by measuring changes in evoked spinal sympathetic reflexes. Our observations that L-dopa and 5-HTP exert opposite effects on the sympathetic reflex arc suggest that NA and 5-HT descending neurones exert reciprocal control functions on the sympathetic outflow.

In unanaesthetized spinal cats, the left stellate ganglion, preganglionic white rami from T3 and T4, and adjacent intercostal nerves were exposed retropleurally. Maximal polysynaptic reflexes from sympathetic preganglionic neurones were evoked every 4 s by supramaximal stimulation of small myelinated afferent fibres in adjacent intercostal nerves and were recorded as compound action potentials from T3 and T4 preganglionic white rami¹⁰⁻¹³. Reflex sizes were analysed by integrating averages of sixteen or thirty-two consecutive responses with a Fabri-Tek signal averager. Control reflexes were stable for many hours, and their stability for at least 1 h was required before precursors were tested.

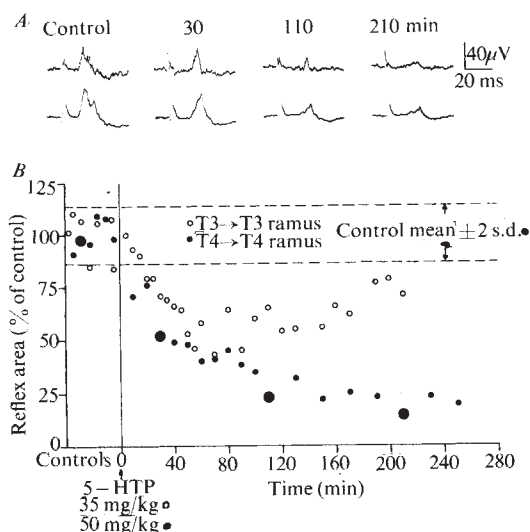


Fig. 1 Depression of cat spinal sympathetic reflexes by 5-HTP. Records in *A* show individual reflex responses (upper trace) and corresponding computer averages of 16 consecutive responses (lower trace) during the control period and at three intervals after administration of 50 mg/kg of 5-HTP. The time course of this experiment is plotted as filled circles in *B*; larger circles represent the sample traces shown in *A*. The open circles in *B* show the depression and partial recovery of a reflex by 35 mg/kg in a different experiment. Control data were collected during at least 1 h.

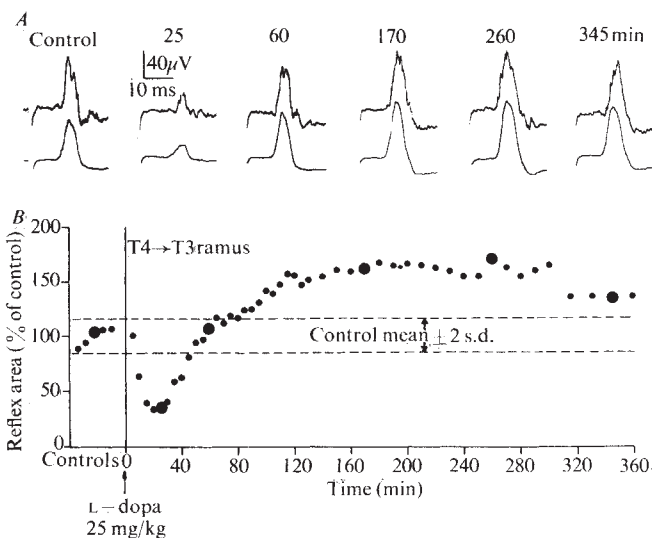


Fig. 2 Depression and enhancement of a spinal sympathetic reflex by 25 mg/kg of L-dopa. Traces in *A* and graph *B* are arranged and constructed as in Fig. 1. The ordinate in *B* is one-half the scale of that in Fig. 1*B*.

Drugs and precursors were injected intravenously. Skeletal muscle paralysis was maintained by gallamine triethiodide ('Flaxedil'). The peripheral vasopressor effect of precursors was controlled by a prior 0.3 mg/kg dose of phenoxybenzamine ('Dibenzylin'). Neither drug affected sympathetic reflexes or their responses to precursors. Blood pressure, end-tidal CO₂, and body temperature were monitored continuously and maintained within optimal limits.

Spinal sympathetic reflexes were invariably depressed by doses of D,L-5-HTP between 25 and 50 mg/kg. As shown in Fig. 1*A* and *B*, 50 mg/kg of 5-HTP led to severe depression of reflexes that persisted for 3–5 h before recovery could be detected; reflexes were depressed to 0–30% of control values. The effects of smaller doses of 5-HTP were less severe and less prolonged. After 35 mg/kg (Fig. 1*B*) reflexes were depressed to about one-half and significant recovery occurred within 3 h; after a minimally effective dose of 25 mg/kg recovery was complete in 1.5 h. The immediate precursor of 5-HTP, L-tryptophan (100–200 mg/kg), also depressed reflexes to 65–80% of control, but the onset was slower and the duration of effect was shorter than those seen during equivalent depression by 5-HTP.

The severe depression of sympathetic reflexes after 5-HTP and, to a lesser degree, after L-tryptophan indicated an inhibition of transmission through the spinal reflex pathway, possibly by depressing excitability of preganglionic neurones. The gradual onset of maximal depression was compatible with slow conversion of precursors to 5-HT within 5-HT terminals leading to its release onto preganglionic neurones. No significant release would be expected before precursor loading since descending 5-HT neurones were decentralized by spinal transection.

L-Dopa (15–50 mg/kg) produced a biphasic effect on spinal sympathetic reflexes (Fig. 2*A* and *B*). After an initial period of marked depression that was maximal within 20–30 min, reflexes recovered to control levels within 60 min and thereafter increased in size until reaching maximum enhancement in 1.5–2 h. Enhancement persisted for several hours before declining toward control level. This sequence could be repeated by a second dose. Doses of L-dopa between 25 and 50 mg/kg initially depressed reflexes to 10–30% of control; 15 mg/kg depressed reflexes by only one-fifth. The secondary, prolonged phase of reflex enhancement ranged between 125–200% of control values in all experiments. Although enhancement was approximately dose related, the large variability among individual experiments probably depended upon differences in the number of additional preganglionic neurones recruited into

a maximal sympathetic reflex. Enhanced reflexes were rapidly reverted to control levels by chlorpromazine (3 mg/kg).

The initial depression of sympathetic reflexes by L-dopa and its similarity to the effect of 5-HT precursors may be explained by recent observations that L-dopa, after conversion to dopamine, rapidly releases 5-HT from central 5-HT terminals^{14,15}. The time of maximal 5-HT release by L-dopa is consistent with the time course of reflex depression by L-dopa in our experiments when allowance is made for the different routes of administration (intraperitoneal¹⁴ or *in vitro*¹⁵ compared with intravenous). Furthermore, the pattern of reflex depression after either L-dopa or 5-HTP was remarkably similar: early components of the reflex were depressed more severely than later components (compare Figs. 1A and 2A).

More direct supporting evidence for release of 5-HT by L-dopa was obtained from experiments in which animals were partially depleted of 5-HT by chronic pretreatment with parachlorophenylalanine (PCPA)¹⁶, 100 mg/kg/day for 3 days before the day of the experiment, or by acute pretreatment (3 h) with reserpine (10 mg/kg). In these experiments the initial depression of sympathetic reflexes by L-dopa was only one-third or less of that seen in animals not depleted of 5-HT. However, the typical enhancement was unaffected except for an earlier onset in the PCPA-treated animals. On the other hand, the depressant effect of L-dopa was markedly intensified and prolonged after pretreatment with minimally depressant doses of 5-HTP. Therefore, the direct correlation between the level of 5-HT available and the degree of reflex depression by L-dopa implicates release of 5-HT as a probable mechanism whereby L-dopa exerts its transient depression. Release of 5-HT by L-dopa may also complicate interpretation of the mechanisms whereby both L-dopa and 5-HTP enhance somatic monosynaptic reflexes and depress somatic polysynaptic reflexes⁴⁻⁸.

The marked and prolonged enhancement of spinal sympathetic reflexes by L-dopa indicates facilitation of transmission through the spinal reflex pathway, presumably by increasing the excitability of sympathetic preganglionic neurones. This facilitation can be attributed to increased synthesis of catecholamines (dopamine, NA or both) and their spontaneous release from the terminals of decentralized NA-containing neurones onto preganglionic neurones. Prompt reversal of the enhancement by a central adrenergic blocking agent, chlorpromazine, also supports such a mechanism.

In addition to preganglionic neurones, the interneurons through which polysynaptic sympathetic reflexes are transmitted are a second potential site of action for released monoamines. If the monoamines formed from precursors influence interneurons non-selectively, then their effects on somatic and sympathetic reflexes should be equivalent since both are mediated through similar polysynaptic pathways that may include some common interneurons¹⁷⁻¹⁹. Both L-dopa and 5-HTP, however, only depress classical somatic polysynaptic reflexes in the spinal cord⁴⁻⁸, whereas they exert opposite effects on spinal sympathetic reflexes. Furthermore, enhancement of sympathetic reflexes by L-dopa by an effect on interneurons is inconsistent with observations that spinal interneurons, including those activated by afferent fibres evoking sympathetic reflexes, are only depressed by NA applied iontophoretically^{20,21}. The histochemical impressions that monoaminergic terminals make "intimate contact" with small efferent neurones in intermediolateral columns^{1,2} also favour preganglionic neurones as a more likely site of action.

Our experimental evidence does not agree with the reported sensitivities of sympathetic preganglionic neurones to iontophoretic application of NA and 5-HT^{21,22}. These investigators found 5-HT to be excitatory whereas NA was either ineffective (spinal cats) or weakly inhibitory (anaesthetized cats). The preganglionic neurones so affected by iontophoretically applied NA and 5-HT may represent a different population than those involved in spinal sympathetic reflexes; in the iontophoretic studies responsive preganglionic neurones were

identified by antidromic activation, but they could not be activated synaptically by a variety of afferent inputs.

In conclusion, we have shown that spinal sympathetic reflexes are depressed by 5-HTP or L-tryptophan and are enhanced by L-dopa. Mediation of these effects by the formation and release of their respective monoamines from the numerous bulbospinal 5-HT and NA neurones terminating in close proximity to sympathetic preganglionic neurones seems likely. This interpretation provides a basis for suggesting that inhibitory 5-HT neurones and excitatory NA neurones exert reciprocal, descending control on the spinal sympathetic outflow. Such a role for NA is supported by recent studies in which neurogenic hypertension following sinoaortic denervation is attributed to increased activity in bulbospinal noradrenergic neurones^{23,24}.

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Heart Rate-Cloacal Temperature Hysteresis in *Iguana* is a Result of Thermal Lag

REPTILES reportedly possess physiological mechanisms for the increase or decrease in the rate of heat exchange with their environments. This the animal could do by changing either its metabolic rate or its effective insulation. Both sorts of mechanisms have been postulated responsible for this capability.

Change of heart rate with temperature has long been regarded an Arrhenius effect, and it is interesting to witness its interpretation as an index of thermoregulatory function. Bartholomew

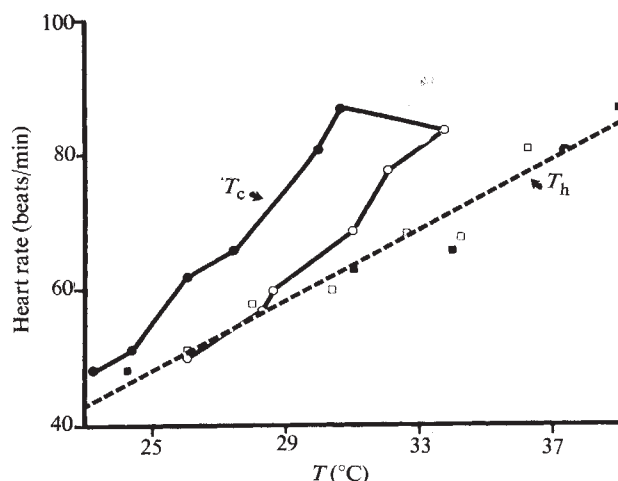


Fig. 1 Graphic comparisons of the effects of temperature on heart rate in *I. iguana*. Solid lines represent heart rates at given cloacal temperatures. ●, Heart rates during heating; ○, heart rates at given heart temperatures; ■, heart rates during heating; □, during cooling.

and coworkers¹ have noted the hysteresis of the heart rate during cooling of several lizards when compared with heating and have claimed that this difference evidenced active control. Although Weathers *et al.*² have shown that this effect is sympathetically mediated, Weathers and White³ claim that turtles, at least, derive from it no thermoregulatory advantage. We have observed a similar asymmetry in our records from *Iguana iguana* and suggest that our asymmetry is a simple hysteresis effect due to the thermal inertia of the tissues.

The purpose of this communication is to point out the inherent difficulty of interpretation of cloacal temperature measurements in physiological experiments.

Iguanas were obtained from Tarpon Zoo, Tarpon Springs, Florida, and were kept indoors with daily light cycle and available water from 1 day to 2 weeks before experimentation. They were fastened to a board in the standard manner⁴ and heated with an infrared lamp at various distances and angles from the body. One thermistor was implanted in the pericardial region after liberal application of 2% 'Xylocaine' and another was inserted 10 cm into the cloaca. Thermistor leads were connected to a YSI telethermometer box and then to the d.c. low level preamplifiers of a Grass polygraph. Electrocardiogram (ECG) leads were attached to the forelimbs and tail. ECG was simultaneously recorded through an ECG pre-amplifier on the Grass polygraph. Results were plotted as heart rate against heart temperature and heart rate against cloacal temperature.

The hysteresis seen with heart rate and cloacal temperature is to be contrasted with the linearity observed when heart rate was expressed as a function of heart temperature.

We have reported the variability of time constants with thermal inertia of the tissue whose temperature is measured

in *I. iguana*⁵. That the thermal inertia of the cloaca exceeds that of the heart seems straightforward enough when blood flows to the two regions are considered, and it therefore seems reasonable to predict that recording solely from the cloacae of animals in experiments involving effects of external variables on internal responses should be interpreted with caution.

Time constants of heating and cooling depend on size, blood flow and insulation of the regions compared. In smaller animals such a measurement of cloacal temperature may not be so misleading, as it may not be in experiments involving high rates of blood flow.

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Pre-courtship Activity and a New Scent Organ in Butterflies

MALE butterflies of the South American genus *Antirrhea* have up to six scent organs, which interact in pairs to form three functional systems. Two were known nearly 100 years ago¹, but the third, which apparently involves pre-courtship activity² and the production of unique dust scales, analogous to similar phenomena studied in the Danaids^{3,4}, has not been described. This discovery raises doubts concerning the higher taxonomy of the Satyrids, and the validity of hypotheses linking the occurrence of male scent organs with mimicry in butterflies.

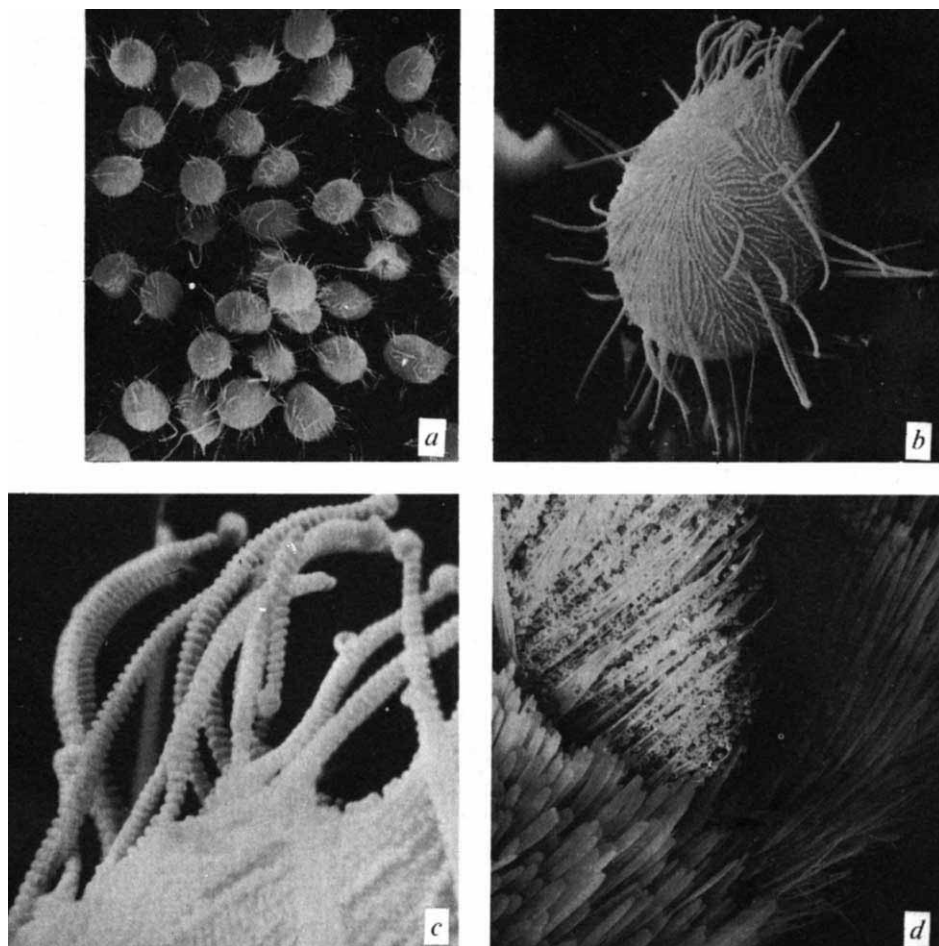
The males of most *Antirrhea* species have a specialized area (Müller's 'opaque patch'¹) on the upper surface of the fore wings. Normally inconspicuous (Fig. 1a), this area may, however, be brightly contrasting (Fig. 1b), owing to a dense covering of minute, yellowish scales of peculiar structure (Fig. 2a). Each scale is roughly spherical, some 35 µm long, with a surface of nodulose ribs from which annulated tendrils arise (about 15 µm long, 0.5 µm thick) (Fig. 2b), with clubbed tips (Fig. 2c). For reasons to become apparent, these scales are here termed *transfer scales*.

Males of such *Antirrhea* species also possess paired, eversible brushes of coremata scales situated antero-laterally to the



Fig. 1 *Antirrhea philoctetes*. a, Male with specialized fore wing area devoid of transfer scales. b, Male showing specialized area of fore wing covered in yellow transfer scales.

Fig. 2 Scanning electron micrographs of specialized transfer scale structures. *a*, Transfer scales removed from fore wing ($\times 253$). *b*, Single transfer scale ($\times 1,145$). *c*, Detail of *b* showing tendrils ($\times 5,710$). *d*, Partly everted abdominal brush organ, showing coremata covered with transfer scales ($\times 30$).



genitalia, analogous to the abdominal hair-pencils found throughout the Danaids⁵. If these brush organs⁶ are dissected from a specimen (Fig. 1*a*) they are found to be packed with transfer scales. Fig. 2*d* shows a partly everted brush with the transfer scales *in situ* on the coremata. If, however, a specimen as in Fig. 1*b* is dissected, no transfer scales are found in the brushes. Evidently the special scales are transferred from the fore wings to the abdominal brushes, probably soon after emergence, possibly by passing the abdomen back and forth between the closed wings, with the brushes everted. The individual coremata, owing to their structure, may expand and contract, which together with the transfer scale tendrils, provide an adherence and release mechanism. The males of certain Danaid butterflies are known to pass scented "dust" particles to the antennae of prospective mates during courtship⁴; such a mechanism would enable *Antirrhea* males to use their unique transfer scales in a similar manner. Direct observations are, however, lacking. Apart from the intrinsic interest of this system in *Antirrhea*, the present discovery has significance in two broader contexts.

Abdominal brush organs, similar to those of *Antirrhea*, occur in the closely related genus *Caerois*, and are also to be found in all species of *Morpho*⁷. Although *Caerois* and *Morpho* lack transfer scales, these scales are also absent in some *Antirrhea* species, which none the less retain their abdominal brush organs. Ultrastructure and behaviour studies may be able to establish whether or not the abdominal brushes common to these three genera are truly homologous. Certain larval similarities, and the discovery that *Antirrhea* and *Morpho* have strikingly similar and unusual hemispherical eggs, suggest that the close relationship of these genera is a useful working hypothesis. If this proves so, it will seriously weaken ideas recently advanced concerning the Biinae as pivotal to the evolutionary history of the Satyridae⁸; preliminary investigations strongly suggest that its component groups, the Biini, Melanitini and Antirrhini, in no way constitute a valid mono-

phyletic group. The validity of the present Morphinae⁵ is likewise questionable.

It has been suggested that to develop Müllerian mimicry, butterflies require complex male scent apparatus, to overcome the potential difficulty of visual recognition between species⁹. Similarly it was suggested that sex-limited mimicry in butterflies arises in the absence of such apparatus, but such selection is unlikely¹⁰. Further, the occurrence throughout the butterflies of Müllerian mimicry, sex-limited mimicry, unimodal Batesian mimicry¹¹ and sexual dimorphism does not clearly correlate with the occurrence of complex scent organs in a manner to be expected from such hypotheses. The study by Brower⁹ omits a number of Müllerian groups such as the *Actinote*, *Acraea* and *Mylothris* and unimodal mimics such as *Pseudacraea* which lack complex scent apparatus. Also omitted are sex-limited mimics such as *Pareronia* which have conspicuous scent organs. Further, certain genera—an example is *Catonephele*—showing striking sexual dimorphism throughout include species with complex scent organs which are closely related to others without. Scent organs apparently play a vital epigamic role in those species which possess them⁴, and have been evolved many times^{1,13}. There seems to be no simple connexion between their presence or absence and the ability to become sexually dimorphic, polymorphic or mimetic in any particular fashion. If a connexion exists, it is more subtle. In this regard, *Antirrhea* species, all of which are non-mimetic, are worthy of attention, since they apparently rival the frequently studied, aposematic Danaids for complexity of scent organ structure and function.

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Thyroid of the Coelacanth, *Latimeria chalumnae* Smith

THE thyroid of bony fishes is composed usually of thyroid follicles more or less scattered in the ventral pharyngeal area, although two additional variations in thyroid structure have been found in teleosts. Earlier reports have described the aggregation of thyroid follicles to form a discrete thyroid mass lacking a fibrous capsule, so that additional follicles located elsewhere are not uncommon. It was later revealed that functional thyroid follicles may be scattered not only in the pharyngeal region but also heterotopically in areas such as the lymphoidal pronephric rudiment (head kidney). With the capture in the Comoros of a small (85 cm) living *Latimeria chalumnae* on March 22, 1972¹, it became feasible for the first time to define the thyroidal structure and the relationship of this gland in terms of osteichthyan or tetrapod characteristics.

A suspected thyroid mass was dissected from the coelacanth and fixed in Bouin's fluid at the site of capture. The paraffin embedded material was sectioned serially at 3, 5, 7 and 10 μ m. The sections were stained with PAS-iron haematoxylin, Mallory's trichrome, aldehyde fuchsin-trichrome, toluidine blue² or Ungewitter's urea-silver nitrate procedures.

The material proved to be thyroid tissue. The orientation of the tissue at the time of fixation was not defined so that it is not possible to indicate anatomical relationships. The gland is irregularly elongate and measures approximately 1.8 mm in diameter by 4.8 mm in length (see Fig. 1). It is embedded in loose connective tissue and in adipose tissue as these surround the gland. Furthermore, it is incompletely encapsulated by thick, dense collagenous connective tissue. This capsule varies from 40 to 90 μ m in thickness. Connective tissue septa extend from the capsular connective tissue and ramify to divide the gland incompletely so that it is somewhat lobulate. An artery obliquely traverses the thyroid and branches to supply the follicles. Interfollicular capillaries and lymphatic vessels are present. Nerve fibres were not found in the limited number of sections available for study. Each of the follicles is supported by AF positive reticular fibres and fine collagenous fibres. The size range of follicles containing colloid is from 28 to 560 μ m (see Fig. 2). Nests of thyroid cells are also abundant. These nests originate in the larger follicles, many

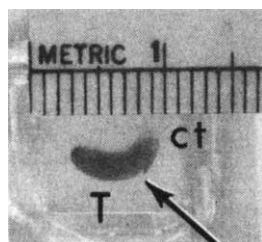


Fig. 1 The mass suspected to be *Latimeria* thyroid tissue. The arrow indicates the separation of the thyroid tissue (T) on the left from the connective tissue (ct) on the right. The entire mass is embedded in paraffin for sectioning.

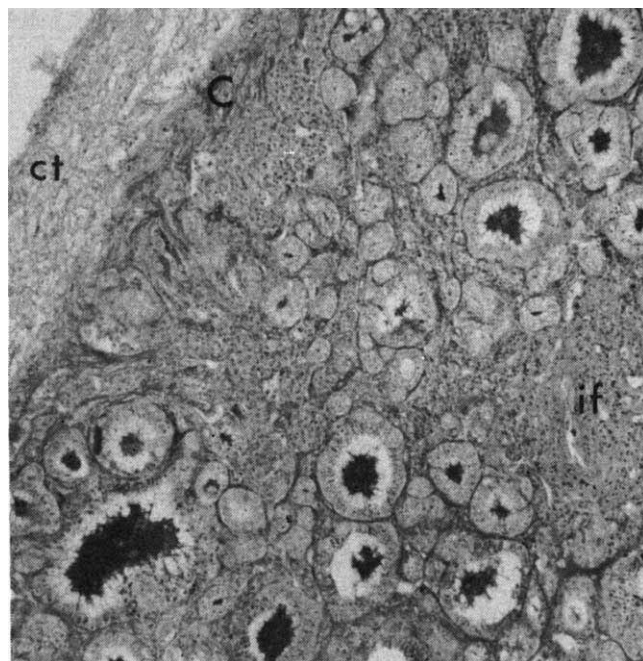


Fig. 2 Peripheral area of the *Latimeria* thyroid. The loose connective tissue and some adipose cells surrounding the thyroid are at the upper left (ct). The dense connective tissue (C) which partially encapsulates the thyroid occurs between this aerolar connective tissue and the thyroidal tissue. The great variation in follicular size is evident. The activity of the follicular epithelium is clear even at this comparatively low magnification. Masses of interfollicular cells (if) are present in several areas. PAS-iron haematoxylin. Section thickness, 10 μ m. $\times 100$.

of which can be seen to be in the process of budding one or more smaller follicles or nests. The follicular epithelium appears highly active. The follicular cells are generally high columnar with an epithelial height of $19.39 \mu\text{m} \pm 0.79$ ($\bar{X} \pm \text{s.e.m.}$). The oval ($8.4 \times 5.6 \mu\text{m}$) to spherical ($8.4 \times 8.4 \mu\text{m}$) nuclei are basally oriented indicating epithelial cell polarity. The nuclei contain a prominent nucleolus, fine chromatin granulation and a faintly staining nuclear membrane. The very slightly basophilic granular cytoplasm contains small chromophobic vacuoles and numerous colloid droplets, the largest of which are 2 μm in diameter. The droplets are both acidophilic and basophilic but all are PAS positive and AF positive (see Fig. 3). Droplets are also frequently present in the cells forming the thyroidal cell nests despite the absence of a follicular lumen. Many of the intracellular colloid droplets stain with toluidine blue, but the thyroglobulin does not react in this technique. The thyroglobulin itself is highly vacuolated peripherally and appears to be both inspissated and dilute as it may be acidophilic and/or basophilic. The follicular colloid is PAS positive and AF positive. The larger follicles usually show considerable cellular debris in the colloid. Parafollicular cells are not present, but a number of cell types are present in the interfollicular spaces. Areas of the thyroid are filled with large masses of these cells. Although at least one interfollicular cell type is silver nitrate positive, none of the interfollicular cells stain with toluidine blue. Thus, thyroidal C cells, if present, are not identifiable.

The thyroid gland of *Latimeria* appears to be compact and encapsulated, thereby differing from the numerically preponderant actinopterygian portion of the osteichthyan line. Other work in our laboratory, dealing with the lower sarcopterygian thyroid, established the early origin of the crossopterygian and tetrapod thyroid in the Dipnoil. The thyroid follicular structure of *Latimeria* is similar to other vertebrates including the Actinopterygii. The large follicular epithelial cells appear highly active, thereby suggesting a heightened endocrine activity at the season of capture. As thyroidal C

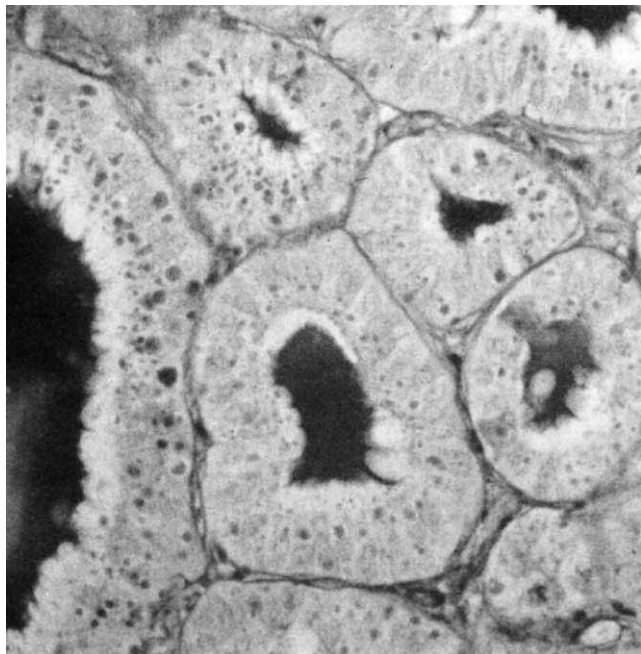


Fig. 3 Thyroid follicles of *Latimeria*. The variation in follicular size is evident. The active high columnar thyroid epithelial cells show the large number of various sized colloid droplets in the cytoplasm. The basally oriented vesicular nuclei have a single prominent nucleolus. Vacuolation of the colloid appears in follicles of all sizes. The connective tissue surrounding the follicles, interfollicular capillaries and interfollicular cells are present. PAS-iron haematoxylin. Section thickness 10 μ m. $\times 400$.

cells were not identified, the ultimobranchial gland may be a discrete structure in the Crossopterygii.

I thank the Royal Society and the members of the French-British-American expedition to the Comoros Islands for their generosity and care in providing tissue from the coelacanth. This work was supported by a grant from the National Science Foundation, Division of Biological and Medical Sciences.

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Visual Pigment of the Coelacanth

DURING the Franco-British-American expedition to the Comoro Islands (January 1–March 28, 1972), two coelacanths were taken by native fishermen. This report of the visual pigment is based on measurements made on a quarter of one retina of the second fish, an immature female, 0.86 m long¹.

The fish was caught at 2.00 a.m. on March 22 at a depth of 165 m, 600 m from the beach at Iconi. The fish was caught in the dark, but it had been light for about 2.5 h when the eye was removed by Dr Lockett at 9.00 a.m. This took about two minutes and the eye was placed immediately in a light-tight freezing cabinet where it slowly froze.

Transport back to the UK without refrigerants was difficult, but use was made of the aircrafts' unpressurized compartments where the package was exposed to freezing temperatures at altitude. On arrival at Sussex it was stored at -30°C until April 10.

The eye was removed from its tin under red illumination. It was ellipsoidal, roughly 44 mm in horizontal width, 40 mm in vertical width and 37 mm in depth. The front of the eye, containing the iris, was cut off by fretsaw, bisecting the deep-

lying lens, which was about 15 mm in diameter. The posterior half of the eye, which contained the retina, was roughly circular in cross section (38 mm horizontal, 35 mm vertical). It was divided in two by a further vertical cut. One half was then bisected horizontally to yield two quarters, one of which was used for extraction of visual pigment. All other parts of the eye were then replaced in the deep freeze in light-tight containers.

After thawing the quarter retina was removed, washed in pH 7 buffer and shaken with 2 ml. 35% sucrose solution. The mix was then centrifuged to precipitate the retinal debris, and the supernatant suspension of photoreceptor outer segments withdrawn and retained. Three more suspensions were prepared by shaking the debris with further portions of sucrose solution and repeating the procedure. All four suspensions were diluted with 4 ml. of pH 7 buffer and centrifuged to precipitate the outer segments. The supernatants were pipetted off, examined, and discarded. The outer segments were washed into one tube with 0.5 ml. of pH 7 buffer and, after centrifuging and removal of the wash, were extracted with 0.5 ml. of 2% digitonin solution.

The absorbance spectrum of the extract showed that only a little pigment was present; it was decided therefore to measure the changes, if any, caused by a total bleach with non-isomerizing light, rather than to risk a programme of partial bleaching experiments.

Neutral hydroxylamine solution was added to the extract until it was approximately 0.02 M. The absorbance spectrum was measured, and after a 15 min exposure to white light passed through a Wratten 15 filter was measured again. The difference spectrum (Fig. 1) shows that the visual pigment has λ_{max} at 473 nm and is based on retinal. The positive portion of the difference spectrum agrees with a 473 nmogram curve, which is some evidence for the homogeneity of the coelacanth pigment.

The low yield of visual pigment ($\Delta A_{\text{max}} = 0.023$, path length 10 mm, volume 0.52 ml., quarter retina) is not consistent with the rich yields usually obtained from deep-living fishes, and is probably due to the exposure to daylight¹ and failure of pigment regeneration after death. Nevertheless, the trace of pigment left in the retina has been sufficient for characterization.

Most deep-living fishes and deep-diving mammals have visual pigments with λ_{max} in the range 477–487 nm (refs. 2–14). Only one fish¹⁵, an elasmobranch, has been reported with a

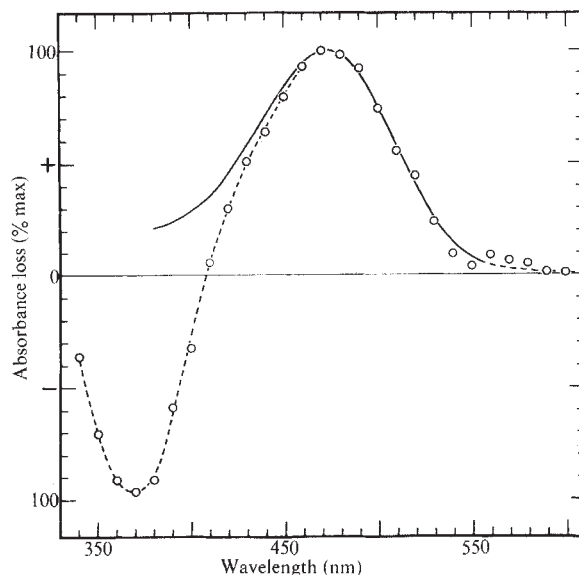


Fig. 1 Normalized difference spectrum (actual $\Delta A_{\text{max}} = 0.023$) of the coelacanth visual pigment (broken curve through symbols) in the presence of 0.02 M hydroxylamine. The product band (λ_{max} = about 370 nm) is characteristic of the oxime of retinal. Continuous curve is for a 473-nm "nomogram" pigment.

pigment of lower $\lambda_{\max}$. This is the deep-sea shark *Centroscymnus coelolepis* which has the value 472 nm. The coelacanth with pigment $\lambda_{\max}$ at 473 nm thus shares with this animal the lowest spectral position recorded for a principal pigment. I report here a similar (474 nm) retinal-based pigment in the eye of the castor-oil fish *Ruvettus pretiosus* (collected from Grande Comore) which lives at the same depths as the coelacanth. The pigment is associated with a smaller amount of another retinal-based pigment of $\lambda_{\max}$ around 485 nm.

The visual pigments of *Centroscymnus*, *Latimeria* and *Ruvettus* are possibly identical—at least in a spectrophotometric sense. They are remarkably precise adaptations to sunlight filtered through a considerable depth of oceanic water. Below 100 m such light is generally maximal between 470 and 476 nm (refs. 16–18). This correspondence supports the "Sensitivity Hypothesis"¹⁸ that under conditions where the light level is very low the best visual performance will be mediated by a visual pigment the absorbance maximum of which coincides with the peak of the ambient light.

I thank Dr N. A. Locket, Mr G. R. Forster, Dr Q. Bone and the other members of the expedition for their assistance on Grande Comore, the Coelacanth Research Committee of the Royal Society (Chairman, Dr F. C. Fraser), and Mrs Erika Church for help with the preparation and measurements reported here.

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Ion Permeability of Phospholipid Bilayers

PHOSPHOLIPID bilayers are an important part of biological membranes^{1,2} and consequently their physico-chemical properties have been the subject of intense investigation. There has been much discussion about how similar the properties of phospholipid bilayers are to those of membranes^{3–5}. Because of the importance of ion transport across membranes (such as in action potentials), the question of the similarities of the intrinsic ion permeability and conductance has been raised. However, reliable information on these properties of the unmodified bilayer is scarce and the situation is confused. Here we present, for the first time, accurate sodium and chloride ion permeability measurements across both charged and uncharged unmodified phospholipid bilayers.

Earlier ion permeability studies^{6,7} using both unsonicated and sonicated liposomes⁵ were troubled by ignorance of the total lipid-H₂O interfacial area and fluxes, and permeability coefficients could not be determined. Papahadjopoulos and

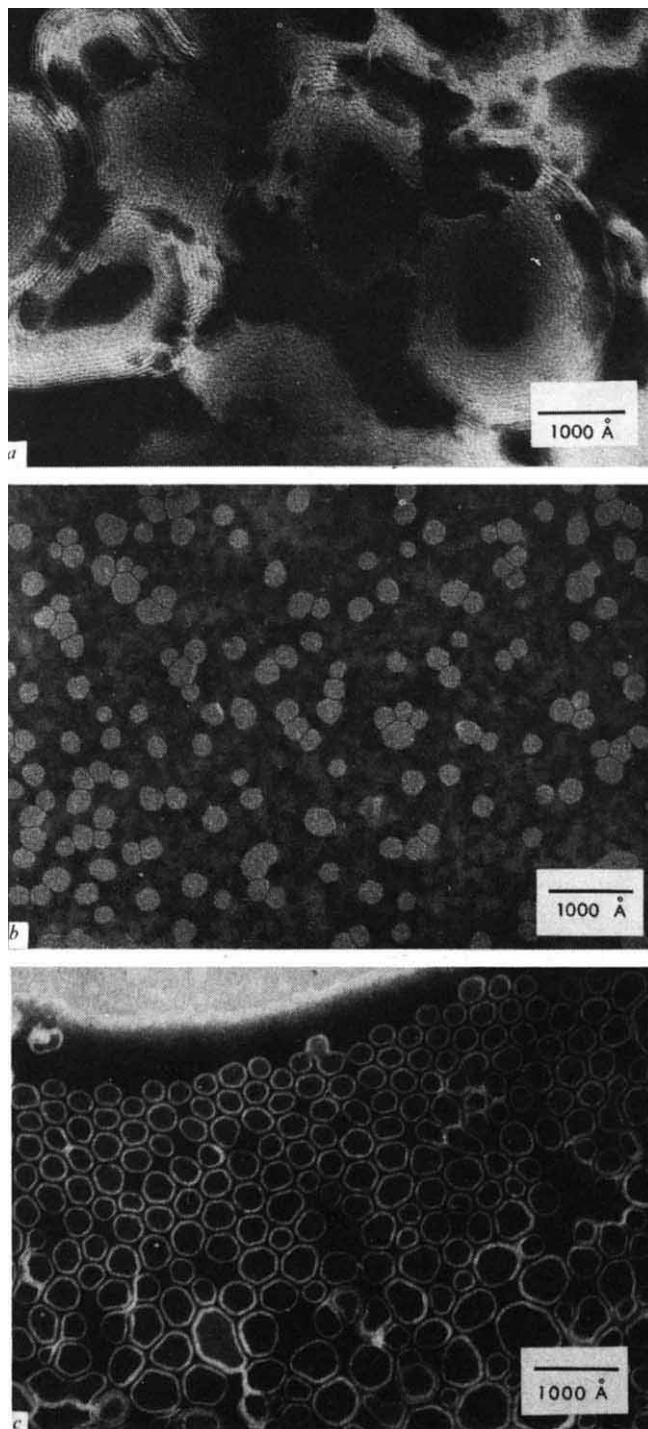


Fig. 1 Electronmicrographs of negatively stained (potassium phosphotungstate, pH 7) specimens of (a) unsonicated egg lecithin dispersion, (b) egg lecithin vesicles and (c) egg lecithin vesicles after adding alamethicin at a concentration of 10^{-3} M (molar ratio egg lecithin to alamethicin = 9 : 1).

Watkins⁷ assumed a lipid-H₂O interfacial area of 900 cm² per μ mol of lipid and calculated K⁺ and Cl[−] fluxes across egg lecithin bilayers as 5×10^{-16} and 3×10^{-14} mol/cm²/s, respectively, with a flux ratio of $M_{Cl^-}/M_{Na^+} = 60$. Unfortunately the liposomes used in these early studies contained multilamellar structures which have serious disadvantages. In addition to those mentioned by Johnson and Bangham⁸ and Bangham¹¹, we found that some of these large particles can contain defects (see ref. 9) through which the negative stain potassium phosphotungstate penetrates (Fig. 1a). Also, reduction in their size by sonication leads to a decrease, rather than the expected increase in the percentage of ions diffusing

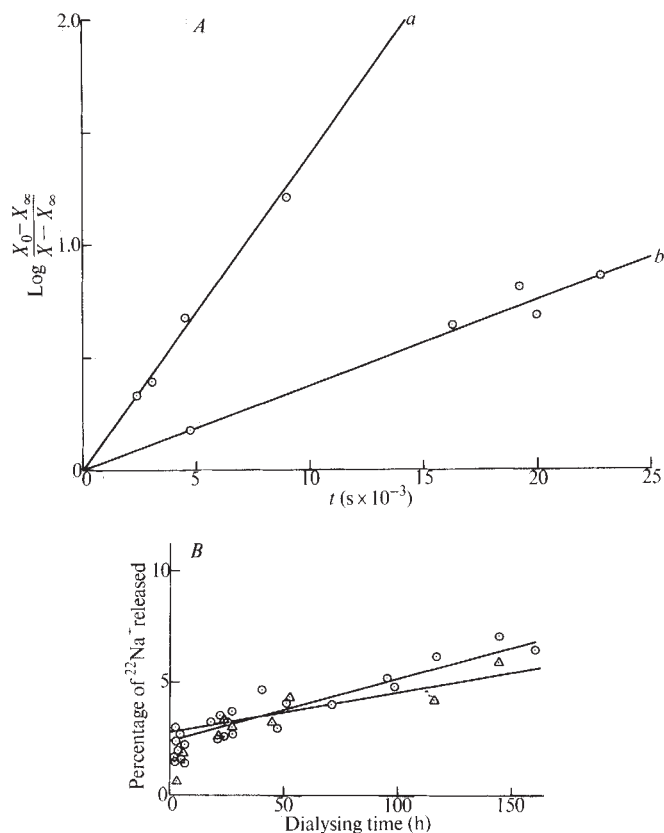


Fig. 2 *A*, $\text{Log} \frac{X_0 - X_\infty}{X - X_\infty}$ plotted as a function of time for Cl^- diffusion out of egg lecithin (*a*) and oxbrain phosphatidylserine (*b*) single-shelled vesicles. The symbols are defined in the text. *B*, Because of the much slower rate of diffusion of $^{22}\text{Na}^+$ the data are more conveniently presented as the percentage of $^{22}\text{Na}^+$ diffusing out of the vesicles with time which is related to the rate constant k by:

$$\text{percentage } ^{22}\text{Na}^+ = 2.5 + \frac{100}{X_0} [X_0 - \text{anti log} (\log X_0 - kt)].$$

○, Egg yolk lecithin; Δ, oxbrain phosphatidylserine.

out in a given time⁷. Thus, although this system might be suitable for measuring relative permeabilities, it cannot yield intrinsic ion permeabilities of the bilayer because exchange of ions with the exterior does not only occur by diffusion across the bilayer. Johnson and Bangham^{8,10} found that prolonged sonication improved the situation and they measured the K^+ flux at 37° C out of "double-walled liposomes" of egg lecithin-4% phosphatidic acid: it ranged between 4.2×10^{-17} and 1.3×10^{-16} with a mean of 8.5×10^{-17} mol/cm²/s.

Recent nuclear magnetic resonance studies have proved that

vesicles produced by sonication are surrounded by a single bilayer and sealed¹²⁻¹⁴. Fig. 1*b* shows that, in contrast to the multilamellar particles, such vesicles are impermeable to the negative stain. Stain can only penetrate into these vesicles when they are deliberately ruptured by sonication in the presence of negative stain. The penetration of the stain can be even more clearly demonstrated by electron microscopy when the bilayer permeability is increased by incorporation of certain antibiotics (Fig. 1*c*). Measurements of the efflux of ions from the interior of single-shelled vesicles could ideally give an accurate intrinsic ionic permeability for phospholipid bilayers. In view of the inconsistencies in the previous ion permeability measurements, there is clearly a need for reliable data on sodium and chloride permeabilities. This communication describes such measurements using a well defined system^{13,15} containing a single class of phospholipid molecules.

The diffusion of Na^+ and Cl^- ions across hen egg lecithin and ox brain phosphatidylserine bilayers was measured at 4° C (to reduce lipid decomposition) in the absence of a concentration gradient ("passive diffusion") with the interior of the phospholipid vesicles containing a ^{22}Na or ^{36}Cl -labelled solution of 0.145 M NaCl (pH 5.5-6.0 and activity 5-20 μCi/ml.). The same results were obtained when both $^{22}\text{Na}^+$ and $^{36}\text{Cl}^-$ were incorporated into the same vesicle preparation and the Na^+ and Cl^- fluxes determined simultaneously. Details of the preparation of sonicated phospholipid dispersions have been described before^{13,15}. The particle size analysis of our single shelled egg lecithin vesicles indicates an average vesicle diameter of 250 ± 8 Å (about 4,000 molecules per vesicle), a lipid- H_2O interfacial area of $3,000 \pm 30$ cm²/μmol lipid and an internal volume/μmol of lipid of $0.33 \pm 0.01 \times 10^{-3}$ cm³ (determined experimentally from the percentage of radioactivity trapped). The particle size analysis of single-shelled ox brain phosphatidylserine vesicles shows that to a first approximation the figures for egg lecithin can be used.

Table 1 summarizes our permeability measurements. The ion fluxes and permeabilities were calculated from the data in Fig. 2. The diffusion for both ions followed first order kinetics⁶ so that the rate constant k could be calculated from the equation

$$\log \frac{(X_0 - X_\infty)}{(X - X_\infty)} = \frac{kt}{2.3}$$

where X_0 , X and X_∞ are the concentrations of radioactive ions incorporated in the vesicles at time = 0, t and at equilibrium, respectively. k is related to the flux M by $k = MA/[Na^+]V$ where A is the mean outer vesicle surface area, V the internal vesicle volume and $[Na^+]$ the ion concentration in mol/cm³. The rapid increase in activity of the dialysate (Fig. 2*B*) observed during the first 5 h amounted to 2-4% of the total radioactivity trapped within the lecithin particles and is tentatively attributed to leak from a small percentage of multilamellar particles and/or some "faulty" vesicles.

Table 1 Na^+ and Cl^- Permeation out of Phospholipid Vesicles at 4°C

Parameters related to permeability	Egg lecithin		Ox brain monosodium phosphatidylserine		Human red blood cell (ref. 17)
	Cl^-	Na^+	Cl^-	Na^+	Cl^-
Self diffusion rate*	$2.3 \dagger$	2.5×10^{-2}	$5.4 \times 10^{-1} \dagger$	9.5×10^{-3}	
Ion flux (mol/cm ² /s)‡	8.0×10^{-15}	1.7×10^{-18}	2.1×10^{-15}	6.5×10^{-19}	1.4×10^{-8}
Permeability coefficient (cm/s)	5.5×10^{-11}	1.2×10^{-14}	1.5×10^{-11}	4.5×10^{-15}	2×10^{-4}
Rate constant k for diffusion (s ⁻¹)	3.2×10^{-4}	8.0×10^{-8}	8.7×10^{-5}	2.3×10^{-8}	3.1
Conductance g_{ion} , (ohm ⁻¹ cm ⁻²)§	3.2×10^{-8}	6.8×10^{-12}	8.4×10^{-9}	2.6×10^{-12}	5.4×10^{-2}

Figures given in the table are the average values of three to six experiments. The ion flux measurements are accurate to within half an order of magnitude.

* Expressed as percentage of counts dialysing out during 1 h.

† At zero time.

‡ The ionic composition was the same on both sides of the bilayer and passive diffusion rather than a net flux was measured.

§ According to Hodgkin¹⁸ the ion flux M_{Na^+} is related to the conductance $g_{\text{Na}^+} = F^2 Z M_{\text{Na}^+} / RT$ where F is the Faraday constant, Z is the valence of Na^+ , R is the universal gas constant and T the absolute temperature. The electrical conductance G of the bilayer is related to $g_{\text{Na}^+} = t_{\text{Na}^+} G$ and $g_{\text{Cl}^-} = t_{\text{Cl}^-} G$ where t_{Na^+} and t_{Cl^-} are the transference numbers of Na^+ and Cl^- in the bilayer, respectively.

The cation and anion permeabilities given in Table 1 are significantly lower than the equivalent data of Papahadjopoulos and Watkins⁷ and Papahadjopoulos¹⁹. The K^+ flux measurement of Johnson and Bangham⁸ is comparable to our Na^+ value for egg yolk lecithin in Table 1. In the light of the above discrepancies, it must be stressed (see ref. 11) that to obtain meaningful permeability coefficients using the single-shelled lipid vesicles in sonicated dispersions the following criteria must be satisfied. (1) The sonication has to be carried out as reproducibly as possible under conditions such that chemical degradation^{15,16} of the lipid is kept at a minimum. (2) The particle size distribution has to be determined. (3) Any multilamellar structures have to be separated from the single-shelled vesicles.

Despite the many similarities between the physical properties of phospholipid bilayers and cellular membranes⁴, Table 1 shows that the ion permeability of bilayers is one property which bears no resemblance to that of cell membranes. The conductance of cell membranes is at least four–ten orders of magnitude higher than the intrinsic bilayer conductance (Table 1) and hence, where unmodified lipid bilayer is part of a cell membrane, its contribution to the total membrane conductance is negligible. Nevertheless, the knowledge and determination of the ion permeabilities of phospholipid bilayers is important because this information is necessary as a basis for quantitative investigation of both the effects of additives on this intrinsic bilayer property and the resultant mechanisms of ion transport.

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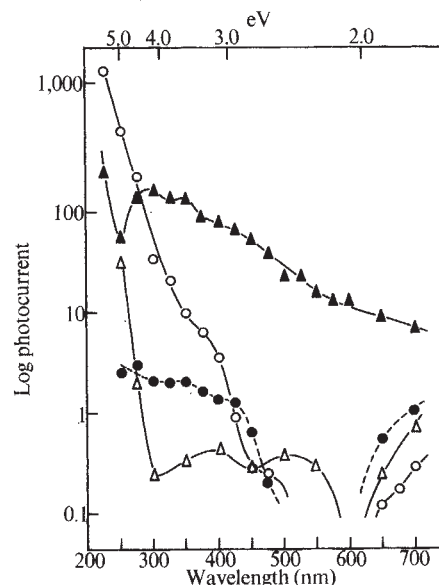


Fig. 1 Ultraviolet and visible photocurrents. $\blacktriangle$, Beef heart mitochondria, surface cell, 23° C, 1×10^5 V m⁻¹, $R \approx 1 \times 10^{11}$ Ω ; $\triangle$, cytochrome c, surface cell, 100° C, 1×10^5 V m⁻¹, $R \approx 1 \times 10^{14}$ Ω ; $\bullet$, haemoglobin, sandwich cell, illuminated electrode positive (hole photocurrent) 50° C, 5×10^5 V m⁻¹, $R \approx 1 \times 10^{12}$ Ω ; $\circ$, haemoglobin, sandwich cell, illuminated electrode negative (electron photocurrent) 50° C, 5×10^5 V m⁻¹, $R \approx 1 \times 10^{12}$ Ω ($\rho = 5 \times 10^{13}$ Ω cm).

Nelson⁵ has measured the action spectra of haemoglobin and gelatin *in vacuo*, and observed a similar wavelength dependence for both photoconduction and photoemission with a " λ_0 " threshold near 3.9 eV. Vladimirov and Timofeyev⁶ found a similar spectral dependence of photocurrent for thin films of actomyosin and trypsin. Liang⁷ has observed photocurrents on illumination of polyglycine I and II with a tungsten lamp. No spectral dependencies for protein photocurrents at below 3.9 eV have been reported.

We have observed non-transient photocurrents in samples of protein and mitochondria not only in the visible but also in the infrared. Measurements were made on pressed 13 mm disks, *in vacuo*, using platinum electrode surface cells for horse heart cytochrome c (Miles-Seravac) and heavy beef heart mitochondria (prepared by Dr E. J. Mayer, Biochemistry Department, Nottingham University), and using a conducting quartz sandwich cell for haemoglobin (Sigma). In the surface cells the electrode length was 10 mm and the electrode gap 1 mm. Sample thicknesses were 0.16 mm (mitochondria), 0.20 mm (cytochrome c) and 0.25 mm (haemoglobin).

In the visible and ultraviolet, two types of photocurrent were generally observed. At energies above 5 eV (mitochondria), 4 eV (cytochrome c) or 3.5 eV (haemoglobin with illuminated electrode biased negative) photocurrents rose steeply and approximately exponentially with photon energy. This was not found for haemoglobin with the illuminated electrode biased positive, although on reversing the polarity we found the effect much larger than for the surface cells. Radiation at these wavelengths is strongly absorbed at the sample-electrode interface so we can say that electrons are the dominant carriers. These photocurrents are similar to those observed with DNA samples which we have investigated in more detail (our unpublished work), and the process of carrier generation is thought to involve the electrodes. The photocurrent maximum which is generally found between 3 and 4 eV in these biological macromolecules has also been observed for DNA where it has been shown that carrier generation occurs independently of the electrodes, because a similar action spectrum is obtained using an electrodeless microwave photoconduction technique. No photocurrents in the 4 to 5 eV region were observed in DNA using the electrodeless technique. Initial work with filters on photocurrents in cytochrome c using the electrodeless tech-

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Photoconduction in Proteins

SZENT-GYÖRGI¹ has reported large conductivity increases in illumination of protein films stained with coloured dyes. Riehl^{2,3} found a photoconductivity in gelatine on 366 nm irradiation and Douzou and Thullier⁴ reported a photocurrent in serum albumin films on illumination with visible light.

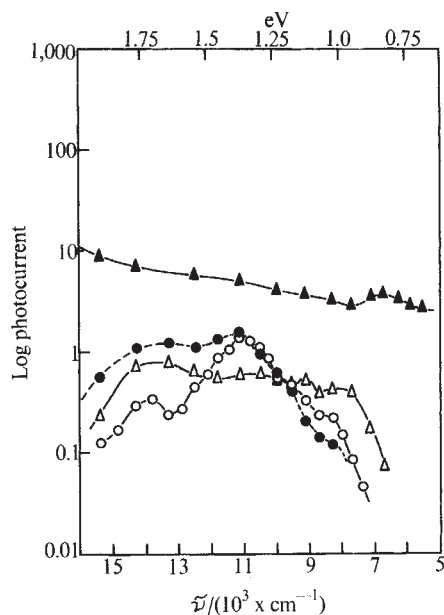


Fig. 2 Infrared photocurrents. Symbols the same as Fig. 1.

nique indicates both infrared and visible-near ultraviolet photocurrents but little or no far ultraviolet photocurrent. Thus it seems likely that the maxima at 4.1 eV for the mitochondria, and 3.1 eV and 2.5 eV for cytochrome c are a property of the samples only, as are the electron photocurrent below about 3.5 eV and possibly the high energy (above 2.5 eV) hole photocurrent for haemoglobin. The photocurrent minima for the two individual haemoproteins near 600 nm occur at energies just below the α -peak solution absorption due to the haem groups. The α and β absorption peaks (500–550 nm) appear to correlate roughly with a maximum in the cytochrome c action spectrum, and the more intense γ peak at 400–450 nm. appears to correlate with the other cytochrome c maximum and the increase in haemoglobin photocurrent.

The only structure evident in the infrared action spectrum for the mitochondria is a small peak near 0.83 eV, although higher resolution studies may reveal more detail. Both cytochrome c and haemoglobin show a peak at 1.71 eV. Haemoglobin shows a further peak at 1.38 eV and a threshold λ_0 of 1.27 eV, while cytochrome c exhibits possible structure at lower energies and a λ_0 near 0.9 eV. These infrared photocurrents in the 1 to 2 eV region thus have marked structure, but are at energies lower than that of the first strong absorption band. Similar near-infrared photocurrents have been observed for metal free and copper phthalocyanines by Day and Price⁸ using an ultra-high vacuum technique, even when the crystals contained very little oxygen and free radical impurity, indicating that the effect was probably an intrinsic property of the phthalocyanines. The photocurrent structure was very similar to that observed in the weak crystal absorption bands⁹, which they considered were probably the lowest π - π^* triplet transitions. In view of the structural similarity of the haem group to the phthalocyanine molecule, and the similarity of the near infrared photocurrent action spectra, it seems likely that a similar mechanism of carrier generation occurs and that the photocurrents are due to absorption at the haem part, not the protein part, of the molecule. If the initial step were triplet absorption one would expect this to be followed by triplet exciton migration and dissociation into an electron and hole at a specific trap that could arise either from impurities or at specific sites within the haemoprotein molecules. Alternatively the structure may correspond to direct excitation of charge carriers from traps. In either case it seems possible that these traps are associated with the redox levels, associated with haem groups¹⁰, (and copper atoms in cytochrome oxidase in the mitochondria¹¹), and that excitation from the traps or of the excitons at the

traps is to a transport band. To examine this working hypothesis further measurements are in progress.

The resistivity of the haemoglobin sample was a little lower than expected for a dry sample indicating that the sample, which had only been evacuated at 50° C or less for 2 days, still contained some water. This was confirmed by the measured activation energy of dark conduction ΔE of only 2.4 eV (using the relation $\sigma = \sigma_0 \exp \frac{-\Delta E}{2kT}$) under approximately these conditions, indicating about 2 to 3% hydration¹². The cytochrome c which appears to lose water very rapidly on evacuation was considered to be "dry" when the action spectrum was measured, after about a week under vacuum at various temperatures up to 100° C. The conduction parameters were in reasonable agreement with those previously published for dry proteins¹³. The water content of the mitochondria sample which was only evacuated for 2 days at room temperature was not known, although the dark conductivity was ohmic in this sample as in all other samples, which suggests a fairly dry sample.

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Grass Evolution, the Kranz Syndrome, ¹³C/¹²C Ratios, and Continental Drift

THE Kranz syndrome is a group of related anatomical, cytological and physiological characters reported one way or another in about 10 families of angiospermous plants. The syndrome centres around the CO₂ fixation aspect of photosynthesis, including synthesis of C₄-dicarboxylic acids as the initial products of CO₂ assimilation, utilization of most of the available CO₂ producing a high ¹³C/¹²C ratio, having maximum efficiency at high temperatures, and all related to a sheath around the bundles in the leaf. All present evidence indicates that detection of any one of the aspects of the syndrome denotes the whole syndrome¹. The syndrome is uncommon among plants including angiosperms and is therefore considered to be the evolved or derived condition. Although in *Atriplex*² it seems to have evolved recently within the genus, in the Gramineae the evolution of the Kranz syndrome seems to have occurred only two or three times early in grass evolution. This assumption, which is fundamental in this discussion, is based on the uniformity of tribes and subfamilies as Kranz or non-Kranz, and the proven predictability of the syndrome from single aspects of it (in this work from the ¹³C/¹²C ratio), from various other characters, or from classification³. The only exception reported, of Kranz and non-Kranz species within one grass genus, is within *Panicum*⁴ where the subgenus *Dichanthelium* is non-Kranz whereas the more typical species of the subgenus *Eupanicum* are characterized by the Kranz syndrome (Table 1). Recently we found, by determination of ¹³C/¹²C

Table 1 $^{13}\text{C}/^{12}\text{C}$ Ratio in Panicoideae

Low, -24 to -31 (non-Kranz)*		High, -10 to -16 (Kranz)*	
Isachneae	Paniceae	Melinideae	Paniceae
Olyreae	<i>Amphicarpum</i>	Garnotieae	<i>Echinochloa</i>
Phareae	<i>Ichananthus</i>	Arundinellae	<i>Paspalum</i>
	<i>Lasiacis</i>	Anthephoreae	<i>Pennisetum</i>
	<i>Oplismenus</i>	Andropogoneae	<i>Stenotaphrum</i>
	<i>Sacciolepis</i>		<i>Setaria</i>
	<i>Panicum</i>		<i>Panicum</i>
	<i>Dichanthelium</i>		<i>Eupanicum</i>
	<i>Eupanicum</i>		(fifteen spp.)
	(five spp.)		<i>Paurochaetium</i>

*Numbers refer to $\delta^{13}\text{C}$ in per mille relative to Chicago standard (PDB₁).

ratios of over 180 species, in 110 genera, of forty tribes of grasses, evidence of both types also among species of *Eupanicum* as well as among genera of the tribe Paniceae and among tribes of the subfamily Panicoideae (Table 1). It is this diversity of panicoid tribes (many of which are small and endemic), of the tribe Paniceae, and the genus *Panicum* that provides clues to the likely evolution of the grass family, superimposed, of course, on a great deal of other morphological, anatomical, cytological, and physiological evidence^{3,5}. Furthermore, now that geology has accepted Wegenerian drifting continents and the break-up of an ancient single world-continent (Pangaea) by rifting under the concept of plate tectonics, evolution of the Gramineae can be discussed in terms of that concept; that is, Upper Jurassic-Middle Cretaceous separation of East Gondwana and the Middle to Late Cretaceous separation of South America from Africa, with the tropical belt approximately in its present relationship to the continents since Early Jurassic time⁶.

With respect to the Gramineae, it is assumed that the origins occurred as non-Kranz types during Jurassic or Cretaceous time and that some subfamilial and tribal diversification preceded the break-up of Pangaea. Because all evidence⁷ indicates a basic angiosperm chromosome number of about $x=7$, it is assumed that original grasses (probably all extinct) had $x=6$ because so many of the less advanced tribes have the secondary basic number of $x=12$. Dysploid increase and decrease from $x=12$ produced other Cretaceous-Jurassic grass lines with $x=13$, 11 and 10. Some of these originated in or spread into (1) the northern and temperate part of Pangaea, called Laurasia, consisting of Asia, Europe and North America, and (2) the southern and temperate region of Pangaea, called East Gondwana, consisting of Australia and Antarctica. Their descendants are the small temperate tribes having basic chromosome numbers 10, 11, 12 and 13 (Arundineae, Brachyelytreae, Danthonieae, *Molinia*, Glycereae, Lygeae, Meliceae, Micraireae, Zizaneae, Nardeae, Phaenospemeae, Stipeae and so on). Another group of ancient grasses also developed in the temperate regions but kept the low chromosome number of $x=7$. This line of evolution eventually produced the very successful Festucoideae which are in all ways simple and distinct, but did evolve or maintain large chromosomes.

Other ancient non-Kranz grasses were adapted to tropical regions of West Gondwana (Africa and South America) with chromosome numbers $x=10-13$. Their descendants are the usually small tribes of tropical Africa and South America, the Anomochloae, Bambuseae, Centothecae, Oryzeae, Parianeae, Phyllorachieae, Streptochaeteae, and so on. Among these mid-Cretaceous, tropical, non-Kranz grasses were some of the original Panicoideae including genera of Paniceae and ancient species of *Panicum*. Descendants of the non-Kranz panicoid types are the small tribes Isachneae, Olyreae and Phareae, and possibly Thysanolaeneae, Lecomtelleae, and Boivinelleae (Table 1). The non-Kranz Paniceae have modern generic and specific descendants such as in Table 1 and probably many others. But shortly before the late Cretaceous separation of South America from Africa the Kranz syndrome evolved within

some panicoid type to give descendants in the Old and New World as the small panicoid Kranz tribes Anthephoreae, Arundinelleae, Garnotieae and Melinideae, and probably Arthropogoneae and Trachyeae as well as some Kranz genera of Paniceae and species of *Panicum* (Table 1).

In South America, which was isolated (probably) from all other continents through most of the Tertiary, the non-Kranz but especially the Kranz Paniceae became the dominant grasses, far more so than in any other region of the world⁸. At some later time out of some panicoid Kranz type arose the Andropogoneae in Southern Asia⁹, and quickly occupied Africa. Some Andropogoneae reached North America, probably by way of southern Europe before the Tertiary separation of those continents had been completed, and reached Australia as it penetrated the south-eastern Asiatic island chains.

According to this scheme the non-Kranz North American *Dichanthelium*s are not *Panicum* (which we here define as a Kranz genus) but originated from some non-Kranz panicoid type of South America or the West Indies (which are part of the South American plate) and, after union of North and South America, expanded rapidly in south-eastern United States. Likewise, all other non-Kranz species should be removed from *Panicum*.

The geographical origin of the Eragrostoideae (all Kranz tribes and species) is obscure if one assumes that the continents have always occupied their present positions¹⁰ since there are centres of abundance in most hot dry temperate regions of North America, Africa (Sahara and South), India and Australia, especially in South Africa¹⁰. The Kranz bundle sheath and the chloroplasts within it¹¹ as well as many other differences indicate no close relationship to the Panicoideae¹². The Aristideae, which have two concentric bundles, the inner panicoid and the outer eragrostoid^{12,13}, constitute the end of a distinct Kranz syndrome evolutionary line which, like Eragrostoideae, occupies the hot dry regions of the Earth.

The logical assumption is that the Eragrostoideae and Aristideae evolved from some Late Jurassic to Mid-Cretaceous probably tropical, grass types that very early acquired among other characters the Kranz syndrome. With the break-up of Pangaea they were already on Australia, India, Africa, and South America where they expanded greatly. The geographical distribution of the Eragrostoideae is a classic example of the biogeographic "proof" of Continental drift⁴.

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BOOK REVIEWS

Social Archaeology in the Aegean

The Emergence of Civilization: The Cyclades and the Aegean in the Third Millennium BC. By Colin Renfrew. Pp. xxviii + 595. (Methuen: London, 1972.) £14.75.

DR JOAN EVANS told how Minoan Crete and Knossos particularly became for her brother the kingdom of his mind. The fruits of his excavations of the splendid series of Minoan antiquities on and around the Kephala hill are described in the volumes of the *Palace of Minos*, an unparalleled analysis of Knossos and the entire Minoan civilization. In the years since, a host of books has been devoted to general accounts or particular aspects of the Minoan and Mycenaean achievement. Yet these have been content essentially to describe the prehistoric sequence and the artefacts which comprised it. Colin Renfrew's massive work has a totally different aim: to discover by very detailed analysis of artefacts in the widest sense, namely the whole adapted and created environment, what processes and interactions caused the civilizations of the Aegean to come into being and, further, what were the origins, causes and initial mechanisms of these processes.

In the last twenty years or so at least two differing trends have been observable in Aegean prehistoric archaeology. There has been the spectacular growth of discovery—the Minoan palace of Zakro with its incomparable treasury of vessels in hard and beautiful stones, the fresco paintings and the arresting religious idols of Mycenae, the rich antiquities of Mycenaean Thebes, the temple and clay goddesses on Kea and, above all, the amazing abundance of beautiful objects and wall paintings from beneath the volcano of Thera—which has stimulated discussion and appreciation. Secondly, there have been the investigations of earlier periods, Palaeolithic, Neolithic and Early Bronze Age. Here archaeologists have not merely extended back our knowledge of Aegean lands by several thousand years, but have brought to bear on these ages a whole range of scientific techniques to aid their en-

vironmentalist approach. Plants, bones, stones, clays and metals have been investigated to provide an understanding of the living systems, agricultural, industrial, economic, social, religious, which are the essential basis of any more outstanding cultural achievement. Professor Renfrew has excavated and studied deeply in this second area of activity. His book uses at the same time the knowledge derived from the environmentalist approach to understand the third millennium BC, the Early Bronze Age of the Aegean, during which were created most of the foundations which lead on to the brilliant cultural achievements of the Late Bronze Age.

The core of the author's approach is systems theory. He begins with several chapters of theoretical discussion in which he defines the framework of his argument: what is meant by civilization, how cultural changes and innovations may be explained with a systems model and, from this model, how what he terms the multiplier effect is responsible for growth, "this mutual interaction in different fields of activity, this property of human systems that an innovation (and its acceptance) in one subsystem favours innovation in another, this interdependence among subsystems which alone can sustain prolonged growth, the multiplier effect". He is thus able to avoid the difficulty of single factor explanations of cultural change, for example, rise in population or invention of metallurgy, by stressing the interactions of differing aspects of the whole living system as essential causes of growth.

Part I then sets out in detail the Aegean Neolithic and third millennium cultural sequence. The sections on mainland and north Greece and on Crete are full summaries of what is known, but the long presentation of the east Aegean and Cycladic cultures and their development and wide connexions is almost wholly new work, entirely superseding all previous accounts. The author concentrates on the settlement site of Phylakopi for the stratigraphical sequence (admittedly inadequate as yet

in the middle phases of the Early Bronze Age) and on numerical analyses of the many cemeteries and their graves individually for establishing the chronology.

In Part II, after beginning with a survey of settlement patterns and population changes, he discusses the material remains as subsystems of the whole living system: natural environment and subsistence, the development of metallurgy, specialization in crafts and technologies and their transformation of the environment, social systems, symbolic and protective systems (that is, man's formal expression, as for example in mensuration and writing, of his view of reality, and his projections to deal with the unknown through his technology for the living (religion) and the dead (burial customs)), and lastly the trading and communications subsystems and the innovations they promote. In a final chapter, "The Multiplier Effect in Action," the author does three things. He considers and rejects entirely the principal alternative model for the explanation of cultural changes, namely migratory or more generally diffused influences from outside the Aegean; he sets out the conclusions from his whole analysis, that the changes and growth which were to produce the Minoan and Mycenaean civilizations of the palatial ages were caused by the multiple interactions of the third millennium subsystems; thirdly, he examines the fundamental question of "the precise nature and origin" of the interactions themselves, which he finds in the individual human basis of activity. "The social activities of man are not always expressed in purely social terms. Social hierarchy can indeed be established by personal combat—a purely social form of contact—but more commonly among humans it is determined in other ways. Social actions are effected more often in material terms—by display of material wealth, for instance, or by gifts of valued objects. Out of this equation of material objects and social activities or values, which in cold logic are simply not equivalent, existing as they do in different dimensions, arises the whole

complex pattern of interactions among human activities such as those which we have been describing. Such links among the activities in different dimensions of the human individual make possible the links between the sub-systems of the culture system which allow the operation of the multiplier effect".

Appendices of much value to the Aegean specialist present a gazetteer of Neolithic and Early Bronze Age sites in the Cyclades and discussions of various subgroups of the Early Cycladic cultures. There is a twenty-five page bibliography and an excellent index. The book is fully illustrated with line drawings (though a few have reproduced poorly from other works), diagrams, graphs and excellent distribution maps, and with thirty-two plates, each with several objects or sites. The reproduction of the photographs is good on the whole, but a few are poorly lit or otherwise fail to show detail well.

Apart from the basic instruction which the book provides through the detailed presentation of the early Aegean cultures and their sequences, a wider general value will depend on a student's attitude to the author's whole approach, that the explanation of cultural changes is to be sought in local processes and interactions, particular human responses to the particular environment of the Aegean region. That the major models used, subsistence/redistribution and craft specialization/wealth, show the approach and the conclusions to be basically correct seems to me substantially proved. But one must appreciate that this approach involves the elimination of external (to the Aegean) human factors, especially population movement, as instruments of change and growth. Some students will find this difficult or impossible to accept. I consider that insufficient emphasis is placed on the evidence for major externally introduced changes at two stages, the beginning of the period in Crete, around 3000 BC, and more demonstrably, the major break at the end of Early Bronze 2 in mainland Greece and the Cyclades when there appear cultures essentially new in the area and with close Anatolian and possibly northern links.

Some small points may be selected for comment. To speak of "the final collapse of the palace system in Crete in the later twelfth century BC" (page 47) as a simple statement of fact is, let us say, unwise—no Minoan archaeologist has accepted Palmer's curious views. The pattern burnish styles of EM I Crete and Beşikatepe in the Troad are much closer than is here (page 77) allowed (in Crete the decoration is often reserved and inside the vases just as at Beşikatepe), and in consequence the Anatolian links with Crete are much

closer. Early Minoan II is inadequately defined (pages 84–85); Vasilike ware should not be taken as the defining new feature (it is not common until late EM II): rather it is hatched painted decoration and the fine grey incised wares which are new at the start of EM II (compare Zoes, *Eph. Arch.*, 1965). There is no need to doubt the existence of Early Minoan III as a period (pages 86–87), certainly a definable stage in east and north central Crete (compare Hood, *Bull. Inst. Class. Stud.*, 13, 110; 1966; and Zoes, *Proc. Second Intern. Cret. Congr.*, 1, 141–156; 1968). Vasilike and Myrtos are correctly identified (page 90) as village settlements (and there are no special rooms for chiefs); but this seems to run counter, as far as Crete is concerned, to the arguments for and from EB 2 chiefdoms in later chapters. Great credit is due to the author for his recovery of the crucial Phylakopi third millennium stratigraphical evidence from Mackenzie's original Daybooks. On the other hand, in chapter 14, save perhaps for Messenia the survey data seem too flimsy, both in the amount of ground rigorously explored and the uncertainty of dating surface sherds, to support such highly detailed reconstructions of population statistics. Chapters 15 and 16, on environment, subsistence and metallurgy, will impress Aegean workers as outstandingly good.

The presentation of the material evidence is made with such clarity of thought, such knowledge of detail and such elegance of style that no one can doubt the value of the book. If readers further accept, as I do, that the essential questions for any understanding of a civilization are here investigated deeply, and within a highly persuasive framework, then they will find this work enormously stimulating and important, a κτῆμα εἰς ἀεί indeed.

PETER WARREN

Phloem Physiology

Phloem Transport in Plants. By Alden S. Crafts and Carl E. Crisp. Pp. xxii+481. (W. H. Freeman: San Francisco and Reading, August 1971.) £5.90.

HARTIG discovered sieve tubes in the bark of trees a hundred and thirty years ago. Their possible function in the transport of synthesized products from the leaves to other parts of the plant was soon recognized, and since then structure and mode of functioning have claimed the attention of many distinguished botanists. By 1935 the broad structural features of sieve tubes and their main physiological characteristics

had been discovered and mechanisms for the movement had been hypothesized. The subject was then, and has always been, puzzling because the sieve tubes were living and contained viscous contents (the so-called "slime") which seemed irreconcilable with the simplest hypothetical mechanism—a passive flow of solution along them. The study of both structure and function had languished somewhat, however, when O. F. Curtis wrote his well known monograph thirty-five years ago.

The work under review is the first comparable monograph on translocation to appear since then and its publication is an important event in this field, not only because it covers the era since the application of radioisotopes and the electron microscope but because Professor Crafts has been one of the foremost workers in the field during this period.

The authors are to be congratulated on writing such a comprehensive monograph. All aspects of the structure and physiology of the phloem are described with admirable clarity and, as far as I am aware, every piece of work that has made a significant contribution during the past thirty or more years is carefully and accurately abstracted and described. The chapter on the structure of the phloem is well illustrated with excellent electron micrographs, many being drawn from the work of the Californian school of plant anatomists with which the authors have long been closely associated. There is also extensive reference to individual figures in the literature, themselves not reproduced in the book, but which must be seen to appreciate the commentary fully. This is an excellent feature but does present the reader with a problem unless he is reading in a library.

The book is divided into three sections: "Structure-Function Relations", "Experimental Results", and "Translocation Mechanism". This is a logical arrangement in that these sections broadly refer to structural aspects, physiology and hypothetical mechanisms respectively. But the authors have not found it easy to adhere closely to these headings because the significance of the structure of the sieve tube can only be discussed in the light of its physiology and the significance of its physiology must be examined in the light of possible mechanisms. Furthermore, the choice of topics within each section is rather odd. Section I includes chapters entitled "The Structure of the Phloem", "The Path of Translocation", "Phloem Plugging" and "Phloem Exudation", while section II contains "Assimilate Movement", "Transport of Plant Hormones", and "Movement of Exogenous Substances", and section III "Mechanisms Proposed to Explain Transport Phenomena", "Effects of

Environmental Factors", and "Some Quantitative Aspects of Translocation". Arrangement under such headings has inevitably led to repetition; for example, work on phloem exudation, the effects of temperature and water stress, the role of sources and sinks, and constituents of the translocation stream are each dealt with in more than one part of the book. The various sections are really separate essays. Perhaps they represent individual contributions from the two authors.

The book nonetheless bears the unmistakable stamp of Professor Crafts; over the years he has been an unremitting advocate of Münch's pressure flow hypothesis, and this is evident all through the book. In a sentence the views of the authors are: the sieve plate pores are "open" and freely permit a mass flow which is impelled by a gradient of turgor pressure in the sieve tubes between the "source" and "sink" regions of the plant. Every fact is brought to the bar of this concept and judged as to its consistency with it. The authors evince no shadow of doubt that their views are right and this makes the book all the more stimulating. But many other physiologists and cytologists are less sure. P. E. WEATHERLEY

Invertebrate Structure

Textbook of Zoology: Invertebrates. Edited by A. J. Marshall and W. D. Williams. Seventh edition of *A Textbook of Zoology*. Vol. 1. By the late T. Jeffrey Parker and the late William A. Haswell. Pp. xix+874. (Macmillan: London and Basingstoke, May 1972.) £5.70.

THIRTY-TWO years have elapsed since the last (sixth) edition of Parker and Haswell's well-known textbook appeared of which the first edition was published in 1898. This new edition has been edited by the late Professor A. J. Marshall and by Dr Williams with the cooperation of several distinguished authorities on different groups of invertebrates. When the sixth edition was new, its rival was *The Invertebrata* by Borradaile, Eastham, Potts and Saunders, popularly known as "BEPS", and I for one, as a student, much preferred it to "Parker and Haswell". The scene has now changed considerably and this new edition may be compared with the textbooks of Barnes and of Meglitsch. These are written in American of course, and it is pleasant to have once more a new textbook—for this is really what it is, in spite of its historical associations—written in English. There is little doubt that "Marshall and Williams" will win support and the choice between these three excellent textbooks

will clearly be dictated by the type and extent of the course being run. All are specially for undergraduate use to supplement lectures and practicals and to provide an introduction to invertebrate biology by instilling a working knowledge of invertebrate structure and taxonomy.

Several specialists have contributed sections dealing with certain groups. I was particularly pleased to see that the Protozoa have been contributed by Eugene Kozloff, the Platyhelminthes and nemertines by J. Llewellyn, the Cnidaria and ctenophores by Cadet Hand, Bryozoa by J. S. Ryland and the echinoderms by Barraclough Fell. This should be enough to reassure the reader that this is not just a last attempt to prop up an aged text but rather a genuine effort to give to it a new lease of life. Clearly the larger phyla have more devotees, and to single out the chapters mentioned is not to cast aspersions on other sections. How delighted and perhaps amazed Parker and Haswell would have been to see this new edition illustrated with stereoscan photographs of *Ascaris* lips!

Marshall and Williams's book is well arranged and the figures well reproduced in filmset with half-tones and line drawings alongside. It continues the Parker and Haswell tradition of providing a large number of illustrations (there are 803) to give as wide a range of invertebrates as possible. Some of these illustrations are the same as in earlier editions but many of them are new. The book continues the "type" approach: there is much to recommend this for an elementary book which is designed as a basis for further courses on invertebrate biology or physiology. Indeed this is the strength or weakness—according to one's point of view—of the present text. There is very little biology and much morphology. Many students today seem to find courses designed to provide a working knowledge of the invertebrates irksome, if not dull, and the success of this book will clearly depend on how it is used. Taken alone it is somewhat old-fashioned. But it could be an ideal companion for a lecture course in which the biology and physiology were stressed, for one would complement the other. I personally believe that the acquisition of a working knowledge of animal structure and taxonomy is an essential prerequisite for further work, however specialized this may be. Whether this purpose is served best by Marshall and Williams or by one or other of the other textbooks currently available depends on the teachers concerned, but it is quite clear that this new edition will take its place among the two or three in English (and there are only two or three) worth considering.

There are some details with which I

would personally disagree: the prominence given to *Neopilina*, for example, is, I think, unjustified in the present state of our knowledge. The position is correctly stated, but the relative sizes of the figures devoted to *Neopilina* and the "archetypal mollusc" should surely have been reversed. Some other figures are small, but they are at least clear. While most are adequately labelled some are not and this applies particularly to many of the drawings of Crustacea. More serious, I think, is the prominence given to Sharov's theory of the origin of the arthropod. While Dr S. M. Manton subjected Sharov's views concerning the specialized polychaete *Spinther* to the derision it deserved, this is not mentioned, and for students to equate this theory with those expressed by Tiegs and Manton is in my view misleading.

The book excludes the protochordates and in this follows the tradition of Parker and Haswell. No doubt they will be included in volume 2 together with the vertebrates. The insects and the "minor groups"—in their commonly accepted sense—are included.

On the whole the reappearance of this book provides an agreeable experience: a reassurance that many others still regard the acquisition of an appreciation of morphological detail as an essential discipline for any student of zoology. This is a work which most of us who teach will want on our shelves to join the others to recommend to students at different stages of their study. This is a "primer" (for all its faults) for Barrington's book, and is essential for all zoological libraries.

R. P. DALES

Likelihood

Likelihood: An Account of the Statistical Concept of Likelihood and its Applications to Scientific Inference. By A. W. F. Edwards. Pp. xv+235. (Cambridge University: London, March 1972.) £3.80; \$12.50.

How are conclusions reasonably drawn from research data? The pertinence of the question is indicated by the fact that three essentially different answers are represented in the recent research literature of genetics: suppose the probability of data D , on hypothesis H , is given by the known function $P(D|H)$. The likelihood approach to data analysis is based exclusively on the likelihood function $P(D^*|H)$ where D^* is the data observed. The likelihood ratio $P(D^*|H')/P(D^*|H'')$ is interpreted as characterizing numerically the support by data D^* for hypothesis H' as against H'' . Bayesian interpretations are based exclusively on $P(H)P(D^*|H)$, where $P(H)$ is the long-controversial prior

probability distribution. More standard methods are based on $P(D|H)$ differently (through calculations, under respective H , of various characteristics of a method, such as error probabilities of tests and standard errors of estimates). In Bayesian and standard methods the likelihood function has no autonomous interpretation although it appears in technical roles.

Edwards's lively and readable book is the first extended account of the likelihood approach. In it he introduces the area and the likelihood concept and then treats successively more general problems, drawing many examples from genetics research including some of his own. He addresses primarily the working scientist who has some familiarity with statistics, to whom he appeals to judge the likelihood approach by its consequences, particularly in the scientist's own work. Other approaches are discussed primarily to clarify the likelihood approach.

Edwards emphasizes but discusses only briefly the dependence of interpretations of data on the scientific research context: weight must be given to relative simplicities of models and to their plausibilities in the light of background knowledge; but the only general guidance ventured on such points is "eschew dogmatism". Edwards's book will serve well not only to acquaint scientists with the likelihood approach, but also to stimulate interest in the theoretical bases of the various approaches and in critical comparisons of their applications in scientific research.

ALLAN BIRNBAUM

Polynucleotides

Organic Chemistry of Nucleic Acids. Part A. Pp. xvi+1-268. £9. \$19.50. Part B. Pp. xv+269-639. £11. \$23.50. Edited by N. K. Kochetkov and E. I. Budovskii. Translated from the Russian by Basil Haigh. Translation edited by Lord Todd and D. M. Brown. (Plenum: London and New York, 1972.)

THE content of these books is precisely stated in their title. This restriction of subject matter, to exclude all but a passing reference to biological function, has allowed the authors to present, within reasonable compass, a most welcome systematic account of the literature of the organic chemistry of nucleic acids up to mid-1969.

The Russian editors, already known in the West, are well served by a translation, under the most authoritative editors, which is both lucid and of pleasing

style. The quite speedy publication has, however, been made at the expense of several proof errors. The point mutations arising from transliterations of cited authors' names provide light relief but the occasional wrong reference numbers and an error in the van't Hoff equation are more serious. The nomenclature used is familiar to the West, with one exception: in order unambiguously to identify the position of exocyclic substituents the Russian nomenclature has been wisely retained (for example, 6-methyladenosine is called 6-exo-N-methyladenosine) and will, it is to be hoped, become international.

The first volume deals with the more general aspects of the subject by consideration of the structure of nucleic acids. It begins with a brief survey of classification, distribution and primary sequence determination and ends with a chapter on secondary structure. In between we are given an excellent short summary of the conformation of nucleosides and nucleotides and a critical treatment of the electronic structure and reactivity of heterocyclic bases which is admirable for its constant juxtaposition of theory and experimental data. The only inconsistency I found in this multi-author monograph was in this chapter, where, on page 166, we are told that there is no evidence that the C5-C6 double bond of pyrimidines is the most reactive to radicals. That evidence is summarized on page 417! Part A concludes with the infuriating statement that the index is in part B.

It is in part B, examining the special organic chemistry of the nucleic acids, that one's best expectations are fulfilled. A chapter is devoted to each of the various types of reactions of the nucleic acid components with particular reference, wherever possible, to reactions at the polynucleotide level. An excellent introduction to nucleic acid photochemistry completes the volume.

In summary, I think the authors have succeeded in their aim to familiarize the reader with the chemical reactions of the polynucleotides. The best way to commend it to interested research workers is to say it is worth its price.

J. P. GODDARD

Physics in Memoriam

Nuclear, Particle and Many Body Physics. Edited by P. M. Morse, B. T. Feld, H. Feshbach and R. Wilson. Vol. 1. Pp. xxi+693. \$23. Vol. 2. Pp. xxi+931. (Academic: New York and London, March 1972.)

AMOS DE-SHALIT, a distinguished Israeli nuclear physicist, died at the early age of 43 in 1969. At that time he was Director General of the Weizmann Institute of Science, and in his short career had made a profound impression not only on the world of nuclear physics but also on all physicists, whatever their specialization, with whom he had come into contact. In the eulogy which prefaces the two volumes under review and which are dedicated to Amos de-Shalit, Igal Talmi writes, "His death was the loss of a leading figure in nuclear physics, of a mentor and guardian to physics in Israel and of a friendly and helpful colleague to his many friends all over the world".

The deep respect with which he was held led the Weizmann Institute to sponsor the publication of a volume of *Annals of Physics* in which the invited papers were dedicated to his memory. The response to the invitation extended to research physicists to contribute papers was overwhelming and in the end seventy-four papers written by 146 authors were published in volumes 63, 66 and 69 of *Annals of Physics*. The present two volumes collect all the papers together in bound form and as such represent a massive and lasting tribute to him.

It is, of course, impossible to review the individual papers. They range over many fields—theoretical and experimental nuclear physics, elementary particle physics, formal mathematical physics, many body theory and so forth—and the tremendous spectrum amply demonstrates his wide influence. A common feature to many of the papers reflects de-Shalit's own approach to physics, namely, a concern with the underlying physical significance of phenomena. As Professor V. F. Weisskopf writes about him—"he would take up a problem with enthusiasm and with an insight which does not come from special expert knowledge, but from a deep understanding of the real meaning of physics"—or about his attitude—"The true goal of science must be the discovery of the basic laws of nature and the clarification of the essential features of natural phenomena".

Many of the authors are very distinguished and many of the papers are important, but it cannot be said that they all conform with the editors' hope that each author would contribute "one of his more significant papers" to the volumes. It also cannot be ignored that since the papers have all been published in a standard journal it is unlikely that sales of these volumes will be extensive. Nevertheless, Amos de-Shalit's many friends throughout the world will surely wish to possess this memorial to a gifted physicist who was liked and respected by everyone.

R. J. BLIN-STOYLE

Bird Biology

Physiology and Biochemistry of the Domestic Fowl. Edited by D. J. Bell and B. M. Freeman. Vol. 1. Pp. xxii+1-602+cxlv. £10.50; \$29.50. Vol. 2. Pp. xxii+603-1152+cxlv. £10.50; \$29.50. Vol. 3. Pp. xiv+1153-1488+cxlv. £7.50; \$23.50. (Academic: New York and London, December 1971.)

THESE three volumes contain material on anatomy, pathology, immunology, nutrition and various subjects other than physiology and biochemistry of fowl, by more than forty different contributors, all British. The title might better have been "Biology of birds with special reference to domestic fowl," because the volumes duplicate some of the ground to be covered in a four-volume series entitled, "Biology of Birds," to be published by Academic Press. Volume 1 is published and others will soon follow. Several chapters have similar or identical headings in the two sets of volumes. These three volumes on the domestic fowl are edited poorly. Some of the chapters are outstanding, many mediocre, and some are bad; there is duplication because many of the contributors apparently were not aware of what the others were writing.

Volume 1, twenty-four chapters, has topics on structure and function of alimentary canal, and respiration, absorption, intestinal microflora and nutrition, structure and function of kidney, energy metabolism, protein, carbohydrate and lipid metabolism, role of vitamins and trace elements, and endocrine organs except reproduction. The coverage of most of these chapters is fair to good, particularly that of anatomy, some of which—for example, that on respiration and the kidney—is outstanding. The coverage on carbohydrate metabolism and endocrines is extensive but there is some overlapping. Absorption is well done. The treatment of chapters 16-22 (endocrinology) is fair to good.

Chapters dealing with metabolism of lipids and proteins are extensive but cover a great deal on mammals, while the treatment of gaseous metabolism is fair but not extensive.

The second volume includes topics on integument, skeleton, nervous system, muscle, heart and circulation, blood corpuscles, metabolism of red cell, blood groups and coagulation, plasma enzymes, body fluids, plasma glucose, non-protein nitrogen, plasma proteins, lymphoid system, special senses, liver function, and thermo-regulation.

"Structure of Nervous System" by Bolton is sketchy but "Physiology of the Nervous System" is much better, and the chapter on pharmacological action of drugs on the central nervous system is noteworthy. Heart and blood vessel innervation might well have been integrated with chapters 31 and 32. Both

authors describe histo-fluorescent stain techniques for nerves, but apparently were unaware of what each wrote.

"The Heart" by Akester is an outstanding chapter and includes anatomy, ultrastructure and electron microscopic pictures and histo-fluorescent studies of heart (much of his own work) although he might have mentioned the work of Bennett and Malmfors. "The Vascular System," by the same author, and including cholinergic and adrenergic innervation, is particularly good and so is the treatment of the specialized circulation in kidney and liver; but the part on blood flow and cardiac output is sketchy.

The coverage on blood groups by Gilmour is good, and the Lymphoid System by Payne is extensive and deals mainly with the structure and distribution of lymphoid tissue and its role in immunogenesis.

The chapters on Metabolism of Erythrocytes, Plasma Glucose, Plasma Enzymes, and Non-Protein Nitrogen, by D. J. Bell, include much material covered elsewhere by the author in 1965, and others but not mentioned here. "Liver Function" by Clarkson is well done, and the presentation on body temperature is fair but does not cover the review of Dawson and Hudson (1970).

Volume 3 deals in great detail with most aspects of reproduction in the domestic fowl, but the treatment is somewhat long and tedious. The principal author, A. B. Gilbert, wrote eight chapters and points out that "to condense this information into a general survey of reasonable size is formidable"; the authors have not been very successful.

Notable chapters include that on control of ovulation, "Transport of Egg Through Oviduct," "Ovulatory Cycles," and "Male Reproduction" by P. E. Lake, which is a well balanced account with an excellent compilation of information on physical and chemical composition of semen. PAUL D. STURKIE

Woodlice

Woodlice. By Stephen Sutton. Key written in collaboration with Paul Harding and David Burn. Pp. 144. (Ginn: London 1972.) £2.

THE all too rare opportunity to praise a book without reservation is one which Dr Sutton presents to the reviewer. The land-living Isopoda, commonly known as woodlice, have tended to be despised and neglected alike by academic zoologists and by collectors and amateur naturalists, in spite of their evident importance in some ecosystems. Dr Sutton's book should herald the end of such undeserved neglect. It offers, at a very reasonable price and in un-

usually clear and simple English, a remarkably full summary of available knowledge of the group, including its anatomical, physiological, developmental, genetical, behavioural and ecological characteristics, while the "identification" chapter includes first-rate coloured pictures of eight of the twenty-nine native British species, together with line drawings and keys whereby it should be possible to identify almost all of the forty-two species now established in our fauna.

There is an interesting chapter on techniques for the study of woodlice, including practical instructions for a number of simple laboratory and field experiments on species of the group, many of which would be suitable for elementary courses in general biology. At the end the author explains the grid-square recording scheme now being developed for British woodlice, on which currently available distributional information is manifestly very defective. For example, Sutton records *Armadillidium vulgare* as "rare in Scotland", while I have found it in abundance at Burnmouth in Berwickshire, and *A. pictum*, for which Sutton notes "recent records only from the southern Lake District", is common at a number of localities along the Solway coast. The final bibliography is quite extensive, running to six pages, without, of course, pretending to be exhaustive. The weakest part of the book is perhaps the index, which does not include names of woodlouse genera, references to the species outside the "identification" chapter, or authors.

R. A. CROWSON

Atomic Physics

New Directions in Atomic Physics. Vol. 1. *Theory*. Pp. xiii+250. Vol. 2. *Experiment*. Pp. xv+163. Edited by Edward U. Condon and Oktay Sinanoğlu. (Yale University: New Haven and London, April 1972.) Vol. 1, £5.95. Vol. 2, £3.50.

THESE two volumes contain between them seventeen review articles on a variety of topics in atomic physics. They originated as papers delivered at a NATO Advanced Study Institute in Izmir, Turkey.

All such collections risk failure to maintain a constant standard of presentation and intelligibility, and these are no exception. In volume 1, devoted to theoretical topics, the most generally useful contributions are those of D. F. Mayers on "Relativistic Self-Consistent Fields", Oktay Sinanoğlu on "Electron Correlation in N-Electron Atoms—a New Atomic Structure Theory" and P. G. Burke on "Electron-Atom Scattering and Resonances"; this last is a model of clarity. The prize for unintelligibility

must, so far as I am concerned, be given to B. G. Wybourne for his article "Compact Groups in Atomic Physics": doubtless the expert in group theory will learn much, but insufficient introduction is given for the uninitiated even to grasp the main points. Marcos Moshinsky's article on "Application of Group Theory to Problems of Atomic Physics" is much more down to earth, while Eugene Merzbacher gives a most illuminating account of "Second Quantization and Relativistic Effects". Mayers has a second contribution which discusses computational methods in the differential equations of atomic physics, while the techniques of computing are described by Charlotte Froese-Fischer.

Volume 2 contains eight papers on experimental atomic physics, of which four are by Alfred Kastler on various aspects of the spectroscopy of excited states. It is revealing to have the insights of the man who has contributed so much to modern atomic physics, and these four articles are full of good things. It would have been worthwhile to take more trouble with the diagrams, some of which are freehand sketches and many of which have the original lettering in French. The reference lists are so short that they give no adequate coverage of the literature, especially of material published in the last five years. Two of the remaining papers—those by Adnan Şaplakoglu and Richard Marrus—are on atomic and molecular beam experiments. Apart from a "private communication" and a single reference to the abstracts of a conference programme, the most recent reference to original work in Şaplakoglu's paper is dated 1951, and half of the references are to pre-war work; so that "new directions" is not an accurate description of the contents of this article. Marrus's contribution, on the other hand, is far more interesting and up-to-date, and gives among other things an account of the attempt at Yale to measure a charge difference between the proton and the electron. R. H. Garstang provides a workmanlike paper on "Atomic Physics in Astrophysics" which deals most competently with coronal excitation of atoms, with negative ions, and with forbidden atomic transitions. Lastly, C. Nicolaides and O. Sinanoğlu write on "Atomic Transition Probabilities: New Experimental and Theoretical Results and Their Comparison", and give a useful summary of the position concerning allowed transitions.

There is no doubt that some of these articles would have gained substantially had they been submitted to a review journal and hence subjected to assessment by referees; and this is, in my view, a better method of publishing such material.

W. R. HINDMARSH

Critical Points

Introduction to Phase Transitions and Critical Phenomena. By H. E. Stanley. Pp. xx+308. (Clarendon: Oxford; Oxford University: London, July 1971.) £5.

CRITICAL points associated with phase transitions have been familiar features of physics since Andrew's experiments on carbon dioxide in 1869 and Curie's investigations of ferromagnetism at the beginning of this century. Associated with them are singularities in such thermodynamic quantities as compressibility or susceptibility, critical opalescence (or its analogue for neutron scattering), and the anomalous behaviour of such transport coefficients as thermal conductivity, which becomes infinitely large.

The older thermodynamic theories of the critical region, dating back to van der Waals and now called classical, have long been known to be at variance with experimental evidence (qualitatively as well as quantitatively). Their ultimate rejection came surprisingly late, with Onsager's solution of the two-dimensional Ising model problem in 1944. This mathematical *tour de force* paved the way for a picture of the critical region much richer, as well as more complex, than had hitherto been imagined.

Since 1944 there has been tremendous activity over the whole field of critical phenomena, both theoretical and experimental. Out of a rich conglomerate of mathematical work on simplified "models", semi-phenomenological theories rooted in thermodynamics and hydrodynamics, and experimental measurements often of astounding precision, has emerged a new description of the critical region epitomized in the twin phrases "static scaling laws" and "dynamic scaling laws". We still know neither whether these laws are fully warranted experimentally nor, if they are, wherein lies their ultimate theoretical justification; but they certainly provide an effective focus for an immense diversity of current work.

This book is eminently suitable for new workers in this field, whether mathematicians, physical chemists, or physicists. Assuming only minimal acquaintance with thermodynamics and statistical mechanics, it presents a colourful and kaleidoscopic picture sufficiently well organized to allow them to see the wood for the trees. Better introductions exist to sections of this work, but no other book, or major review, has made so ambitious an attempt to cover both equilibrium and the dynamical aspects of it.

It is by no means flawless, and should be approached as a stimulating introduction rather than a definitive Handbuch article. For instance (p. 173), the

"astute reader" confronted with thermodynamically inconsistent formulae ought not, as suggested, to dismiss the results as "inadmissible", but should note where the author erred in deriving them. Its historical asides are episodic rather than well-proportioned (the 1957 paper of Domb and Sykes, the first successfully to extract critical indices from power-series expansions, should certainly have been mentioned). And by restricting attention to systems having only two independent thermodynamic variables the author has excluded some topics (such as "exponent renormalization" due to impurities and "smoothness" postulates) with which even new workers should nowadays be familiar. But these are defects which can be removed from the inevitable second edition (by when, too, there may well be need for revision of some of the experimental data here quoted).

Meanwhile the author's enthusiasm, generosity of outlook, and industry have produced a most worthwhile addition to the literature. And it may appropriately be added that over half of the three hundred and fifty references in this book are to papers published in the last five years.

G. S. RUSHBROOKE

Agronomy

Crop Production: Cereal and Legumes. By Brian F. Bland. Pp. xii+466. (Academic: London and New York, June 1971.) £5; \$14.50.

THIS book provides the reader with a general survey of crop production practices in Great Britain. The first four chapters on wheat, barley, oats, rye and maize would be of principal interest to professional workers and farmers in Great Britain. Later sections on beans, peas and sainfoin provide information that is not readily available from other sources. Each chapter includes a well documented summary of the botanical classification, quality, utilization, field management and pest control for each crop.

Because the chief emphasis is on field management of the crop, the book would be of value as a text for first-year students in agriculture or in a course specifically devoted to agronomy and crop production. Extension workers and farmers would be interested in the detailed information on seed-bed preparation, seed dressing, time of sowing, seed rate, weed and pest control, harvest and economics of production. The reference lists are comprehensive; the writing is clear and concise; the quality of print and illustrations are excellent.

F. W. SOSULSKI

CORRESPONDENCE

Terrorism and Vietnam

SIR,—Your comments in reply to Lester Goldstein (*Nature*, 238, 57; 1972) concerning terrorism and Vietnam raise some very important issues. For you a qualitative difference exists between the terrorist act at Tel Aviv airport and what the US government is doing in Vietnam. The former was such that you felt it warranted to raise a hue-and-cry about individual acts of terrorism in various parts of the world (*Nature*, 237, 302; 1972). (In this also you were selective: you mentioned the IRA while omitting the UDA and the British army.) However, you claim that what is happening in Vietnam, being the "consequence of a declared military policy of a government", is not in "Nature's parish".

Why is this so? What is *Nature's* parish? Would Hitler's gas chambers—a Nazi government policy—and their horrendous use have been outside "Nature's parish"?

Without taking up the question of why the United States government is waging this war (that is, the politics involved), the evidence is overwhelming that what the US is doing is harnessing the most modern concepts in science and technology, on a vast scale, to overcome if not to totally destroy the people and the human environment of a large portion of Indochina. For almost a decade at least, Indochina has been the point of sharpest conflict between advanced technology and the welfare of man and his environment. Never in history has so economically weak and backward a people been subjected to such widespread terror, such destructive energy by so powerful an opponent for so long.

I submit that what the US government is doing in Indochina does fall into "Nature's parish". To hide behind real or imaginary parish boundaries on the issue is no different than to condone the Nazi's genocidal gas chambers. Time is long since past for thinking human beings—including especially scientists and technologists—to view the facts and call for the only possible solution—the immediate and total withdrawal of the US presence from Indochina.

Yours faithfully,

MATTHEW HUSSEY

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Doomwatching

SIR,—Recent *Nature* editorials have suggested, in no uncertain terms, that the present spate of warnings about imminent or future "ecocatastrophe" is alarmist and irresponsible. Of course the excesses of the doomwatchers need to be pinpointed, but by the same token of scientific caution it is appropriate to criticize with equal rigour the optimistic view that science can solve everything; and here *Nature* has been silent.

It should also be remembered that forecasts of disaster, such as the Club of Rome's *The Limits of Growth*, are not wholly negative: they simply point out what are the probable consequences if certain present trends are continued. I do not accept that it is scientifically reprehensible to do this. Indeed, the environmental lobby surely deserves credit for alerting influential individuals and the general public to the full social and ecological consequences of a purely laissez faire attitude to population, resources, and environment. If, and it is a big "if", concerted steps are now being taken to reduce some of the more dangerous trends, then surely this hopeful development owes something to the efforts of the doomwatchers?

The great human disasters have almost invariably arisen from what has not been predicted, and thus has not been susceptible to corrective control. Science must always tend to be conservative, since facts are retrospective; and so necessarily all our activities will have consequences which we cannot confidently predict. Yet the "civilized" world is constantly engaging in enterprises which must have profound future consequences. Their scale is ever-increasing, so that, once begun, they can slowly and painfully, if at all, be reversed. Typically we have only the most fragmentary firm data from which to predict their socio-ecological effects, so there is usually an uncertain but substantial margin of error. One of the few clear lessons of history is that the increasing scale of our activities and decisions yields a concomitant increase in our mistakes and the magnitude of our disasters. Therefore, if we must err (and that we shall do is a very safe prediction), we should surely err on the side of caution, and be excessively sceptical of optimistic rather than pessimistic assumptions. The effects of taking too much heed of the doomwatchers are unlikely to be as harmful as those of taking too little heed.

Since we are already aware of numerous ecological danger-signals, should not *Nature* be pressing hard for more and more research on environmental problems, rather than knocking existing "world models" too hard for their imperfections? Science and technology may be able to overcome many of our difficulties (although no doubt many others will require more fundamental political changes), but only if we recognize what the problems are. More and more, especially if we are to be placed in a "public contractor" role, we shall depend on the publicists who can create the political demand for the relevant research funding. Perhaps then, despite their over-zealous excesses, the doomwatchers deserve a little more of *Nature's* respect.

Yours faithfully,

IAN VINE

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Abbreviations

SIR,—In the August issue of the *British Journal of Haematology* (23, 167; 1972) is a paper entitled: "Detection of Splenic Anti-Platelet Antibody Synthesis in Idiopathic Autoimmune Thrombocytopenic Purpura (ATP)" by S. Karparkin, N. Strick and G. W. Siskind. I wish to point out that the abbreviation ATP is already in universal use throughout the biological sciences for adenosine triphosphate. As such the abbreviation is, of course, also used in publications on platelets. The introduction of the same abbreviation for something else, even for a not too common clinical condition, is likely to cause at best irritation and at worst confusion. I should like to urge, therefore, that ATP should not be introduced as an abbreviation for autoimmune thrombocytopenic purpura. (Incidentally the abbreviation ITP, unfortunately already in common use for idiopathic thrombocytopenic purpura, is also the standard abbreviation for inosine triphosphate. There the disease is already established; what one has to do now is to prevent its spread.)

To substantiate the possibility of confusion I should like to tell the following true story. Several years ago, Dr Malcolm Dixon gave a series of lunchtime lectures at University College on ATP and other "high-energy" phosphate compounds. The lectures had a rather popular title which I have now forgotten,

presumably to attract a wide audience. I do remember, however, that after the first lecture I heard one man say to another behind me: "What an interesting lecturer; but why did he keep referring to Atmospheric Temperature and Pressure?"

Yours faithfully,

G. V. R. BORN

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Ethnic Differences

SIR,—I was pleased to read your unusually well balanced editorial (*Nature*,

239, 1; 1972) on the issue of the freedom of research on possible racial differences in intelligence. However, I am afraid that one important point is missing there; that is the question whether intelligence is the only criterion we should be concerned about with respect to the potential ethnic difference in mentality. There are, I think, many other aspects of possible interest and use concerning racial differences in mental activities; aggressiveness may be one, and sense for subtlety may be another. The reason why these are not on the agenda is due to the large framework-of-value system imposed on contemporary society. We have to admit that in all societies research on certain

aspects of reality is being unconsciously suppressed. In order to realize this, we have to liberate ourselves from the pre-occupation we are all subjected to. What would happen, for example, if it were to be demonstrated that the "quotient of aggressiveness" is significantly higher among one race (say, Caucasians) than others? The stark fact that establishing a scientific measure for aggressiveness has not been explored may simply reflect the inaccessibility to science of those who may be benefited by that type of research.

Yours faithfully,

ATUHIRO SIBATANI

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Obituary

Vsevolod P. Savich

DR VSEVOLOD P. SAVICH, one of the leading Russian specialists on mosses and lichens, died on May 25, 1972.

Savich was born on February 18, 1885. In 1906, while still a student at the University of St Petersburg, he began working as a laboratory assistant at the Imperial St Petersburg Botanical Gardens (now the "V. L. Komarov" Botanical Institute of the Soviet Academy of Sciences) and soon became interested in the lichens and mosses of the Arctic and sub-Arctic—an interest which, like his association with the Botanical Institute, was to last throughout his life.

Savich's main field of work was the cataloguing and mapping of the mosses and lichens of the Russian Empire—later, of the Soviet Union. He took part in a number of botanical expeditions, of which the most notable were the Kamchatka expedition of 1908–1910 and the 1930 expedition aboard the icebreaker G. Sedov to Franz Josef Land, Severnaya Zemlya and Vize Island. His collections during the latter

expedition extended the known range for Franz Josef Land from forty-three varieties of moss and two hepaticae to eighty-three mosses and seven hepaticae. The data from these expeditions formed the basis of his later taxonomic works.

In 1932, Savich founded the Department of Lower Plants of the Botanical Institute, and became the first head of this department—a post which he was to hold for 30 years. Under his direction, the department soon became the leading body for research into these plants in the whole of the Soviet Union. Under his initiative and general editorship, the Institute published a special series of monographs and other fundamental works devoted to mosses, lichens and fungi. He was also associated with a number of more general journals, including *Sovetskaya Botanika*, founded in 1933, of which he and V. A. Keller were the first co-editors, and the semi-popular *Priroda* of which he was editor-in-chief from 1936 to 1951, a post which he relinquished only when the headquarters of the journal were moved from Leningrad to Moscow.

In addition to his editorial work, Dr

Savich was particularly interested in research into possible medical and other uses of lichens. In particular, he studied the application of lichen resinoids in perfumery and the possibility of obtaining antibiotics and other pharmaceuticals from lichens. The latter line of research led to the discovery of 'Binan' (sodium usninate), which for many years now has been used in surgery and gynaecology in the Soviet Union. With L. V. Lazarov, Savich edited, in 1957, a collection of studies on Binan, to which he, himself, contributed a paper "Lichens, their use in medicine and the obtaining of the new antibiotic Binan".

Dr Savich took an active part in the organization of botanical research throughout the Soviet Union. During the 1930s he played a leading part in the organization into a single, unified Botanical Institute of the independent Botanical Gardens and Botanical Museum in Leningrad. He was active as an organizer and editor until his death, working until the end on the current edition of the *Novosti sistematiki nizshikh rastenii*.

Announcements

International Meetings

October 16, **Resistance in Arthropods**, London (Assistant Secretary, Society of Chemical Industry, 14 Belgrave Square, London SW1X 8PS).

October 19, **Non-prescription Antibiotics in Animal Foodstuffs: their Nature and Analysis**, London (F. W. Webb, Central Analytical Laboratories (Biological), The Wellcome Foundation Limited, Acacia Hall, Dartford, Kent).

October 20, **Training of Food Analysts**, Glasgow (Society for Analytical Chemistry, 9–10 Savile Row, London W1X 1AF).

October 25, **Physics of Display Devices**, London (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

October 27, **Geophysical-Statistical Symposium**, London (J. M. Craddock, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ).

November 2, **E. M. Holmes Anniversary Meeting**, London (R. E. Marshall, Department of Pharmaceutical Sciences,

Pharmaceutical Society of Great Britain, 17 Bloomsbury Square, London WC1).

November 2, **Polymers as Catalysts** (Professor Moore Memorial Lecture), Bradford (The Registrar, University of Bradford, Bradford, Yorkshire BD7 1DP).

November 2–4, **Neoplasia of Head and Neck**, Houston (Mrs Jane Brandenberg, Information Coordinator, University of Texas, M.D. Anderson Hospital and Tumor Institute, Texas Medical Center, Houston, Texas 77025, USA).

November 5–16, **Canine Immunogenetics**,

Rotterdam (Mrs H. Zaat-Stevens, Laboratory for Experimental Surgery, University Hospital Medical Faculty, Rotterdam, Netherlands).

November 6-7, **Cholelithiasis — From Theory to Therapy**, Chicago (Dr Daniel Mufson (F70), Smith Kline and French Laboratories, 1500 Spring Garden Street, Philadelphia, Pa 19101, USA).

November 6-8, **The Effect of Maternal Nutrition on the Development of the Offspring**, High Wycombe (Dr Arnold A. Spicer, The Lord Rank Research Centre, Lincoln Road, High Wycombe, Buckinghamshire).

November 6-8, **Cosmetic Science and the Consumer**, Bath (Society of Cosmetic Chemists of Great Britain, 56 Kingsway, London WC2).

November 7, **The Relevance of Tension Test Parameters in Evaluating the Mechanical Behaviour of Polycrystalline Materials**, Birmingham (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

November 8, **Alkylation**, London (Assistant Secretary, Society of Chemical Industry, 14 Belgrave Square, London SW1X 8PS).

November 8, **Reliability Physics of Electronic Components**, London (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

November 8, **Structural Variation in Mammalian Cell Surfaces**, London (Dr R. C. Hughes, National Institute for

Medical Research, Mill Hill, London NW7).

November 13-14, **Nutrition and Foetal Development**, New York (Dr Myron Winick, Institute of Human Nutrition, Columbia University College of Physicians and Surgeons, 511 West 166th Street, New York, NY 10032, USA).

November 13-15, **Geological Society of America Annual Meeting**, Minneapolis (Fred S. Handy, Geological Society of America, PO Box 1719, Boulder, Colorado 80302, USA).

November 13-16, **Weed Control—an EEC Problem**, Brighton (J. A. Cole-Morgan, British Crop Protection Council, 160 Great Portland Street, London W1N 6DT).

November 16, **Industrial Applications of Electron Microscopy and Analysis**, London (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

November 20, **Enhancement of Chemical Measurement Techniques using On-line Digital Computers**, Loughborough (Society for Analytical Chemistry, 9-10 Savile Row, London W1X 1AF).

November 22-25, **Electrochemistry**, Karakudi (Dr N. Subramanian, Central Electrochemical Research Institute, Karakudi-3 (Tamil Nadu), India).

November 22-28, **Launching Bases**, Kourou, French Guiana (Centre National d'Etudes Spatiales, Division des Relations Universitaires, 129 rue de l'Université, Paris 7, France).

November 23, **Occupational Illnesses in the Road and Building Industry**, London (Assistant Secretary, Society of Chemical Industry, 14 Belgrave Square, London SW1X 8PS).

November 23-24, **British Institute of Radiology 75th Anniversary Symposium**, London (British Institute of Radiology, 32 Welbeck Street, London W1M 7PG).

November 24, **Role, Support and Organization of Crystallography in the 1970s**, London (Meetings Secretary, Institute of Physics, 47 Belgrave Square, London SW1).

November 28-December 1, **Magnetism and Magnetic Materials**, Denver (Dr S. Kern, Department of Physics, Colorado State University, Fort Collins, Colorado 80521, USA).

November 29, **The Anxiety State**, London (Dr J. R. Cavalla, John Wyeth and Brother Limited, Huntercombe Lane South, Taplow, Maidenhead, Berkshire).

November 29-30, **Application of Optical Instrumentation in Medicine**, Chicago (Technical Program Committee, SPIE National Offices, PO Box 288, Redondo Beach, California 90277, USA).

November 29-December 3, **Biotelemetry**, Pretoria (Symposium Secretariat, CSIR, PO Box 395, Pretoria, South Africa).

December 4-7, **American Geophysical Union Meeting**, San Francisco (American Geophysical Union, 2100 Pennsylvania Avenue NW, Washington DC 20037, USA).

Notes to Authors

Will contributors to *Nature* follow as far as possible the general style of the journal, and in particular the following rules:

1. Manuscripts should be typed in double spacing and submitted in duplicate. Two copies of all figures should be included, one for the printers and one for the referee.
2. References are indicated by superscript numbers in the order in which they appear in the text, and have the following form:

¹Sargent, W. L. W., and Searle, L., *Astrophys. J. Lett.*, **162**, L155 (1970).

²Fraenkel-Conrat, H., in *Comprehensive Biochemistry* (edit. by Florkin, M., and Stotz, E. H.), **7**, 75 (Elsevier, Amsterdam, 1963).

"Unpublished work", "Private communication" and "Work in preparation" should not be cited as references but may be incorporated in the text. Each reference number should refer to an individual work which has either been published or is in the press.

3. Although the text of longer articles is usually interrupted by headings at suitable intervals, these should be short and informative and not merely "Introduction", "Results", or "Conclusion".

4. Units should either conform to the SI system or be spelled out in full.

A fuller guide can be had from the *Nature* offices in London and Washington.

December 6-7, **Physical Properties of Solids at High Pressure**, Teddington (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

December 8, **Gut Microflora and Nutrition in the Non-ruminant**, London (Dr J. D. Sutton, National Institute for Research in Dairying, Shinfield, Reading RG2 9AT).

December 11-14, **Health Physics Society Meeting**, San Juan, Puerto Rico (Office of the Executive Secretary, Health Physics Society, PO Box 156, E. Weymouth, Massachusetts 02189, USA).

December 13, **Microwave Transistors**, London (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

December 14-15, **R and D for the Chemical and Allied Industries**, Liverpool (W. E. Cowley, The Associated Octel Company Limited, PO Box 17, Ellesmere Port, Cheshire L65 4HF).

December 18-22, **Relativistic Astrophysics**, New York (A. G. W. Cameron, Belfer Graduate School of Science, Yeshiva University, New York, NY 10033, USA).

December 19-20, **Review of Biophysical Research since 1960**, London (Dr A. C. T. North, Department of Zoology, Laboratory of Molecular Biophysics, South Parks Road, Oxford).

Reports and Publications

not included in the *Monthly Books Supplement*
Great Britain and Ireland

The Nuffield Foundation. Twenty-sixth Report, 1971. Pp. 114. (London: The Nuffield Foundation, 1972.) [189]

The Electrical Research Association. Report No. 5275: Successful Predictions of the Electrical Discharge Theory of Cosmic Atmospheric Phenomena and Universal Evolution. By Dr. C. E. R. Brice. Pp. 39. (Leatherhead: The Electrical Research Association, 1968.) [189]

Report of the Review Body appointed by the Council of the University of Birmingham, presented to the Council 21st September, 1972. Pp. 164. (Birmingham: The University, 1972.) [199]
Aslib Occasional Publication No. 10: Data and the Chemist. By Margaret Slater, Averil Osborn and Alexandra Presanis. Pp. 82. (London: Aslib, 3 Belgrave Square, 1972.) £3.50 non-members; £2.90 members. [209]
The Radiochemical Centre Limited. First Annual Report for the year ended 31st March, 1972. Pp. 16. (Amersham: The Radiochemical Centre Limited, 1972.) [209]
Hospital Purchasing. Pp. 30. (London: Office of Health Economics, 1972.) 25p. [219]
Report of the Social Science Research Council, April 1971-March 1972. Pp. 66. (London: HMSO, 1972.) 47p net. [219]
National Physical Laboratory. Changing to the Metric System: Conversion Factors, Symbols and Definitions. By Pamela Anderton and P. H. Bigg. Pp. v+58. (London HMSO, 1972.) 40p net. [219]
VAT—I. Partial Exemption. II. Self-supply (Stationery). (Notice No. 706.) Pp. 16. (London: Her Majesty's Customs and Excise, 1972.) [229]

Other Countries

Australia: Commonwealth Scientific and Industrial Research Organization. National Standards Laboratory—Biennial Report 1969/1971. Pp. 87. (Sydney: CSIRO, 1972.) [189]

US Department of Commerce. National Bureau of Standards Special Publication No. 363: Bibliography on Atomic Energy Levels and Spectra, July 1968 through June 1971. By Lucy Hagan and W. C. Martin. Pp. iv+102. (Washington, DC: Government Printing Office, 1972.) [189]

Israel: National Council for Research and Development. Israel Academy of Sciences and Humanities. The Israel National Committee on Biosphere and Environment. The Environment in Israel. Edited by U. Marinov and E. Harel. Second edition. Pp. 89. (Jerusalem: National Council for Research and Development, 1972.) [189]

National Botanic Gardens of South Africa. Report 1970/1971. Pp. 66. (Kirstenbosch: National Botanic Gardens of South Africa, 1972.) [189]

US Department of Commerce. National Bureau of Standards. Reference Tables for Low-Temperature Thermocouples. By Larry L. Sparks, Robert L. Powell and William J. Hall. Pp. iv+56. (Washington, DC: Government Printing Office, 1972.) 60 cents. [189]

OECD. Science Policy Studies: Problems and Prospects of Fundamental Research in Multi-disciplinary Fields. Computer Science. Pp. 51. (Paris: OECD; London: HMSO, 1972.) 8 francs; 62p; \$1.75. [189]

Australian Journal of Zoology, Supplementary Series, No. 13: Revision of the Larvae of the Tribe Callidosomatini (Acarina: Erythraeidae) with Observations on Post-Larval Instars. By R. V. Southcott. Pp. 84. (East Melbourne: Editorial and Publications Section, CSIRO, 1972.) [189]

Australia: Commonwealth Scientific and Industrial Research Organization. Land Research Series No. 30: Lands of the Aitape-Ambunti Area, Papua, New Guinea. Comprising papers by H. A. Haantjens, P. C. Heyligers, J. R. McAlpine, J. C. Saunders and R. H. Fagan. Compiled by H. A. Haantjens. Pp. 243. (Melbourne: CSIRO, 1972.) [189]

Chlorine: An Annotated Bibliography. (A Selection of references from 1824 to 1971 on the medical, toxicological, industrial hygiene and environmental

aspects of exposure to chlorine.) Prepared by Ralph G. Smith and the Department of Occupational and Environmental Health, Wayne State University, Detroit, Michigan. (New York: The Chlorine Institute, 342 Madison Avenue, 1972.) \$6. [189]

Smithsonian Contributions to Anthropology, No. 16: Environmental Archaeology and Cultural Systems in Hamilton Inlet, Labrador—a Survey of the Central Labrador Coast from 3000 B.C. to the Present. Pp. xix+299 (87 plates). (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) \$3.50. [189]

State of California. The Resources Agency: Department of Fish and Game. Fish Bulletin No. 155: Population Size, Mortality Rates, and Growth Rates of Northern California Ocean Shrimp, *Pandalus jordani*, 1965 through 1968. By Daniel W. Probst. Pp. 47. (Sacramento, California: Office of Procurement, Documents Section, P.O. Box 2091, 1972.) [199]

US Department of the Interior: Geological Survey. Professional Paper 529-J: Atlantic Continental Shelf and Slope of the United States—Petrology of the Sand Fraction of Sediments, Northern New Jersey to Southern Florida. By John D. Milliman. Pp. iv+40+9 plates. (Washington, DC: Government Printing Office, 1972.) [199]

Population Bulletin, Vol. 28, No. 3: Third World Development—Where Does Population Fit? Pp. 24. (Washington, DC: Population Reference Bureau, 1755 Massachusetts Avenue, NW, 1972.) [209]

Publications of the Dominion Astrophysics Observatory, Vol. XIV, No. 3: Catalogue of Theoretical Lunar-Occultation Diffraction Patterns for Single and Double Stars Occulted by the Lunar Limb. By C. L. Morbey. Pp. 45-58. (Victoria, BC: Dominion Astrophysical Observatory, 1972.) [209]

Annals of the South African Museum, Vol. 59, Part 9: A Late Pliocene Rhinoceros from Langebaanweg, Cape Province. By D. A. Hooijer. Pp. 151-191+14 plates. (Cape Town: South African Museum, 1972.) [209]

Journal of Electron Spectroscopy and Related Phenomena, Vol. 1, No. 1, October 1972. Pp. 1-112. Subscription price: Dfl. 90.00 plus Dfl. 8.00 (postage) = Dfl. 98.00; (US\$30.50) per year. (Amsterdam: Elsevier Publishing Company, 1972.) [209]

The Rockefeller Foundation. President's Ten-Year Review and Annual Report 1971. Pp. xv+220. (New York: The Rockefeller Foundation, 111 West 50th Street, 1972.) [219]

Smithsonian Contributions to Paleobiology, No. 13: Upper Miocene Echinoids from the Yorktown Formation of Virginia and Their Environmental Significance. By Porter M. Kier. Pp. 41 (10 plates). (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) 55 cents. [219]

Journal of Geochemical Exploration, Vol. 1, No. 1, July 1972. Published quarterly. Pp. 1-142. (Amsterdam: Elsevier Publishing Company, 1972.) [219]

US Department of the Interior: Geological Survey. Professional Paper 725-A: Physical Environment of the National Reactor Testing Station, Idaho—a Summary. By Raymond L. Nace, Morris Deutsch and Paul T. Voegeli. Edited by Seymour Subitzky. Pp. viii+38+plate 1. \$1. Professional Paper 437-C: Prehistoric Near-Surface Subsidence Cracks in Western Fresno County, California. By William B. Bull. Pp. vi+85+2 plates. \$1.75. (Washington, DC: Government Printing Office, 1972.) [219]

CERN. European Organization for Nuclear Research. CERN 72-14: Intersecting Storage Rings. By E. Keil. Pp. iii+53. (Geneva: CERN, 1972.) [229]

Science Council of Canada. Report No. 17: Lifelines—Some Policies for Basic Biology in Canada. Pp. 73. (Ottawa: Information Canada, 1972.) \$1. [229]

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